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SECOND SERIES VOL. IV, No. 7

BULLETIN

OF

THE NEW YORK ACADEMY OF MEDICINE

JULY, 1928

INSTITUTED 1847

PUBLISHED MONTHLY BY
THE NEW YORK ACADEMY OF MEDICINE
Fifth Avenue and 103rd Street
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Entered as second-class matter, February 3, 1928, at the postoffice at New York, N. Y., under the Act of August 24, 1912.

Subscription $2.00 per year. Single copies, 25 cents.

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PAPERS DELIVERED AT STATED MEETINGS

CONCERNING CERTAIN OCULAR INTERPRETATIONS OF CARDIOVASCULAR, RENAL, METABOLIC AND INTRACRANIAL DISORDERS

(ABSTRACT)

G. E. DE SCHWEINITZ
Philadelphia

Although Charles Babbage, in 1847, devised an instrument for the examination of the interior of the eye, he failed to record an account of it, and hence the invention and introduction of the ophthalmoscope are universally ascribed to von Helmholtz, whose description of his discovery was published seventy-six years ago.

This discovery gave ophthalmology its place among the exact sciences; it opened up a new field of research—medical ophthalmoscopy.

The ophthalmoscope of our day, greatly improved in its mechanism, with electric light as its source of illumination, takes high rank among the instruments of precision whereby investigation and diagnosis of bodily ailments are greatly facilitated.

It is the purpose of the present lecture to endeavor to demonstrate in description and illustration some of its uses in general medical practice.

The chief vascular supply of the retina is derived from the internal carotid through the ophthalmic artery and one of its branches, the central retinal artery.

The retinal arteries do not anastomose with each other; they are end-arteries; they belong, in general terms, to the internal carotid system, and therefore, in certain circumstances, participate in, or are associated with, lesions of the other vessels of this system, that is, those in the brain cortex and basal ganglia.

Arteriosclerosis. The first important association, just referred to, concerns itself with the retinal manifestations of arteriosclerosis, which may be divided into: (a) Those which are suspicious, *i.e.,* justify suspicion of coming events; (b) those which are strongly suggestive, and (c) those which are definite and diagnostic.

The four chief manifestations are alteration of the normal relation of an artery where it crosses a vein—indentation, that is arteriovenous compression; irregularity of the lumen of the vessels; silver or copper wire arteries; tortuosity of the arteries; retinal hemorrhages.

But the ophthalmoscope can do more than simply detect retinal angiosclerosis; it can reveal the variety which in turn indicates the causative factor with considerable exactness. Four classes can be differentiated: (1) *Senile,* (2) *primary,* (3) *consecutive,* and (4) *secondary.* H. P. Wagener's excellent classification is followed.

In the *senile* type there are reduction of the apparent caliber of arteries and veins, but with regular lumina, no arteriovenous compression, but pallor of the disc, sclerosis of the choroidal vessels, and often spots of guttate choroiditis. This condition is nothing more than one which naturally obtains in old age; it may be regarded as a pathological norm. In such an eyeground, naturally, the graver forms of angiosclerosis may develop.

In *primary arteriosclerosis,* that is, endovasculitis, there are exaggeration of the retinal arterial reflex and beading, irregularities in the caliber of the retinal arteries, arteriovenous compression, tortuous terminal arterioles, areas of obliteration of arteries, new vessel formation, perivasculitis, and dusky congestion of the disc.

In *consecutive arteriosclerosis, due to general vascular hypertension*, there are exaggerated retinal arterial reflexes, generalized reduction in the caliber of the retinal arteries, irregularities of their lumina, venous engorgement and *straight arteriovenous compression, i.e.,* the course of the vein is not altered, but beading of the vessels, new vessels, perivasculitis, and spots of arterial obliteration are absent.

Implanted upon a fundus of this character there may be scattered white patches, hemorrhages, constituting the type named by Benedict "retinitis of hypertension plus nephritis," and if the hypertension is malignant, hemorrhages and edema are marked.

Another type, which is engrafted on retinal vascular disease, is *arteriosclerotic retinitis*, especially described by Foster Moore, which should be differentiated from renal retinitis, as shall presently appear, although albumin may be present in the urine. In a fair percentage of the cases it is unilateral. It is characterized by numerous white spots, sometimes collected or fan-shaped, and hemorrhages; the other signs previously named are present.

Recently Wagener has elaborately described the ophthalmoscopic appearances of the retinitis of malignant hypertension which progresses through several stages. Mainly these features are present: Disc-edema, peripapillary edema, hemorrhagic areas, sometimes cotton-wool patches, sclerosis of retinal arterioles. It can be distinguished from other types of vascular retinitis, and is superimposed on pre-existing sclerosis of retinal arterioles. The outstanding pathologic features are marked hypertension, diffuse arteriosclerosis and sclerosis of arterioles and capillaries. The nephritis, never primary, is secondary to arterial disease.

In the *secondary* arteriosclerotic group are included those vascular changes seen after subsidence of renal retinitis and other varieties of retinitis and neuroretinitis and of papilledema.

The early signs of retinal angiosclerosis as described often lead to the discovery of primary arteriosclerosis and vascular hypertension, and may antedate the usual clinical signs—prolongation of the second sound of the heart, the record of the sphygmomanometer, etc., etc.

Excluding senile retinal angiosclerosis, the various types of this disease constitute a fair guide as to the condition of the arteries of the brain, and considerable investigation has developed the fact that in at least 70 per cent. of the cases where retinal angiosclerosis, including arteriosclerotic retinitis, is present, there is a coexisting disease of the vessels of the brain.

In general terms it may be stated that where disease of the retinal arteries is present, as described, there is also disease of the cerebral arteries, but there may be advanced sclerosis of the cerebral arteries and no retinal angiosclerosis.

These patients usually die of vascular cerebral disease, or cerebral hemorrhage or thrombosis; their lives may be prolonged by an early discovery of the retinal changes, and proper treatment.

The next subject for pictorial demonstration are the eyegrounds which indicate renal disease, which are usually denominated albuminuric retinitis—a poor term, for which renal retinitis should be substituted.

Renal retinitis is most common in association with chronic interstitial or glomerular nephritis, and is also seen with parenchymatous nephritis and secondary contracted kidney.

In my experience, in hospital work fully 30 per cent. of the patients with chronic Bright's disease have had various forms of retinitis, but if these statistics should include not only typical cases, but also comparatively insignificant lesions, the percentage would be higher.

There is a general impression among physicians that the retinal indications of nephritis are so definite that they are

well-nigh typical or pathognomonic. This is, however, not always the case. The chief signs are: Edema of the disc, its edges surrounded by a whitish bank of edema, the so-called snow-bank appearance; engorged veins, hemorrhages and a variety of white spots—cotton-wool patches, and fine white spots of degenerated varicose nerve fibers—and dots which coalesce in the macula and form the so-called "star figure." When these, which individually may occur in many conditions, are grouped, the fundus picture is strongly suggestive of renal disease. Marked retinal angiosclerosis may be evident. But there are many types which are not at all like the one described, and yet indicate nephritis. The lesions of the retina are partly due to vascular changes and partly to a toxic influence.

It is an interesting fact that renal retinitis is very often the first definite indication of nephritis, and is discovered by the ophthalmologist, the patient having consulted him entirely unconscious of the fact that he is the subject of Bright's disease.

The presence of renal retinitis renders the vital prognosis most unfavorable, and it is usually stated that patients rarely live longer than two years after its detection. I am sure these figures are too pessimistic, and often with care and proper treatment associated with the advantage of early discovery, the patient's life can be much prolonged.

I am also sure that the various types or manifestations of renal retinitis should be more carefully ophthalmoscopically analyzed. This is work requiring very special knowledge. The prognosis as to life is more evil when cotton-wool patches predominate, and when disc-edema (choked disc) is the only or most conspicuous fundus lesion.

The *retinitis of pregnancy,* usually denominated albuminuric retinitis of pregnancy, occurs most frequently in primiparae, usually in the second half of pregnancy, exceptionally at an earlier period; it is partly due to an associated nephritis, depending on fatty changes in the kidney

epithelium, and partly to toxemic products. The ocular prognosis is much better than that of renal retinitis, and the lesions may disappear with the termination of pregnancy, but if the lesions arise during the first six months of pregnancy, the labor ought to be artificially induced.

The *retinitis of diabetes* occurs in a number of forms. I exhibit two varieties—the one an exudative type with massed whitish exudate in the central area of the retina, hemorrhages and decided vascular sclerosis, and the other with punctate white spots and small hemorrhages, clot or bullet-like; this is the more characteristic type. The white exudates are solid looking (not cotton-wool) and somewhat soapy in appearance.

This retinitis practically never occurs prior to the thirty-fifth or fortieth year of life; often coma, hemiplegia, gangrene, etc., are present. It is quite certain that this retinitis is not due alone to perverted metabolism, diacetic acid, acetone nor hyperglycemia. If it were, young patients would acquire retinitis, which they never do. In many instances, if not in all, angiosclerosis is a concomitant condition and has its etiologic significance. The prognosis is not so evil as that which pertains to renal retinitis.

Tubercles in the Choroid—small yellowish-white spots are usually, although not always, associated with tuberculous meningitis. The spots may be scattered or arise along the vessels; they are usually surrounded by a delicate rose-colored ring. There may be an association of optic neuritis and choroidal tubercle.

Retinal tuberculosis begins with the white formations along the retinal vessels—later perivasculitis and hemorrhages appear, and still later, it may be, proliferating retinitis.

Ophthalmoscopic examination is often of value to decide between a meningitis, for instance, of typhoid fever, and one due to t.b. meningeal involvement. Tubercles in the choroid are usually late in appearance, sometimes just before a fatal illness.

Choked disc (papilledema) is due to increased intracranial pressure which forces the subarachnoid fluid into the intersheath of the optic nerve, causing venous engorgement and lymph stasis.

It is the most important general symptom of conditions of the brain and its meninges, which create this increased pressure—hence tumor of the cerebrum and cerebellum and meninges. It is also caused by many other lesions, but time does not permit their inclusion. Usually the choked disc (papilledema) of increased intracranial tension can be differentiated from optic neuritis (papillitis).

It occurs with all kinds of tumors in all areas of the brain and cortex, basal ganglia and cerebellum (except, perhaps, with pure pontine tumors) and with brain lues. It is present in fully 80 per cent. of tumors of the brain and cerebellum.

Other things being equal, release of increased intracranial pressure by a decompressive trephining is the procedure which promises the best results, that is, prevents blindness from post-papillitic atrophy. It must be done in the early stages to obtain this result. Occasionally repeated lumbar punctures, only a small amount of fluid being withdrawn at each operation, cause a subsidence of choked disc.

All patients with skull injuries should be carefully examined ophthalmoscopically. If advancing papilledema is discovered, it is an indication for operative interference for the purpose of evacuating an epi- or subdural hemorrhage.

The various fundus lesions were portrayed by means of water colors, for the most part painted by the late Miss Margaretta Washington.

THE PRESENT STATUS OF PHYSICAL THERAPY

FRANK B. GRANGER

Boston

During the last few years the medical profession has begun to realize the value of physical measures of treatment.

The impetus which such therapeusis received from its use in the Army, Navy, Public Health and Veterans' Bureau Hospitals, has been increased by civilian agencies.

"The recognition of physical therapy as a legitimate and important branch of medicine by the American Medical Association, and the appointment by it of a Council on Physical Therapy, undoubtedly marked the dawn of a new era in this art."

The Council, composed of clinicians, pathologists, physiologists, biophysicists and physicists, is earnestly attacking the manifold problems which this subject presents. To quote: "The Council on Physical Therapy of the American Medical Association will endeavor to point out to the medical profession the advantages and disadvantages of physical therapy, so that its abuses may be reduced to a minimum and its scientific possibilities may be appreciated." To this end many laboratory and clinical investigations are being made under authoritative auspices. Basic papers on the subdivision of physical therapy—that is, massage, muscle reëducation, electrotherapy, hydrotherapy and mechanotherapy—either have been, or shortly will be, published in the *Journal of the American Medical Association.*

The therapeutic claims, with supporting scientific evidence submitted by the various manufacturers of electrotherapeutic apparatus, are critically examined by the Council. If inconclusive, further evidence is required, or if the subject is of sufficient basic importance it is referred to some medical center with the request that it be submitted to scientific, clinical and laboratory tests. Soon the Council hopes to publish a tentative list of approved

apparatus of those firms whose products are not only scientifically designed and will deliver what they are claimed to, but also whose literature is free from the fairy tales of pseudo-science and impossible biology. In short, the Council on Physical Therapy hopes to accomplish for this subject the same "de-bunking" results which the Council on Pharmacy has consummated for the pharmaceutical manufacturers and those specializing in miraculous patent medicines.

Soon the medical schools will be requested to make such revision of their curricula that adequate undergraduate instruction shall be given in physical therapy. Several plans will be submitted for their consideration. The problems of postgraduate instruction will also be stressed and several solutions will be suggested, such as standardized courses either directly in the medical schools or as university extension courses. Systematic courses under the auspices of State, County, or local societies will be urged and a list of speakers qualified to lecture on certain subjects may be offered. The liberal inclusion of papers on physical therapy in medical conventions will be advised.

When the medical profession at large realizes that the proper application of physical measures of treatment is an effective answer to the challenge of the cults the demand for further instruction will be insistent.

That such instruction is necessary is best shown by the rapidity with which some alleged physiotherapists have been made. Not long ago a one or two weeks' course under commercial auspices was sufficient; now, an enterprising manufacturer advertises that the possession of his piece of electrical apparatus and three hours' study of his compendium of electrotherapy will make its proud possessor a full-fledged physiotherapist (electrotherapeutic branch) competent to cure the ills of man or beast.

The Council on Physical Therapy recently sent questionnaires to seventy-one medical colleges; forty-one reported some instruction in physical therapy; nineteen re-

ported no instruction; two were considering the introduction of such courses; one institution reported that it was organizing a course. A month later three schools reported that courses were being organized, and still three others that since the questionnaire, departments had been formed.

The complaint has been made that the Council has made slow progress and that its work has consisted mainly of promises of what it intends to do. It will very soon be evident, however, that such is not the case. Medicine for centuries was in its swaddling clothes surrounded by the incantations of the medicine man, the soothsayer and the oracle. Even now if we listen to a small but vociferous group we might conclude that medicine had reverted to dream books and interpreters thereof. Surgery also has slowly emerged from the hands of the barber. Much of the mysticism which enshrouded physical therapy and which consequently made it a choice morsel for the quack and the unscrupulous physician has been removed. It is now recognized that one or more of the following properties are inherent in all physical measures of treatment—namely, chemical, thermal, mechanical and psychical. The latter, psychical, has been considered by many to be its only effect. It is true that all therapeusis has greater or less psychical value. It may seem to loom large in physical therapy, but constantly increasing laboratory studies show that relatively it has little more than that which exists in medicine and surgery, and that it is infinitely less than that which exists in much of the modern neurology. Such an attribute is a valuable asset and its possession should not be held up as a reproach against its possessor. In the *Journal of the American Medical Association* it was stated that among the various laboratory investigations the following were significant:

"1. The influence of ultra-violet rays on the blood chemistry, whereby the calcium and the phosphorus content of the blood were generally augmented.

2. The power of diathermy in suitable cases of non-union or delayed union of bone in hastening the formation of callus.

3. The more rapid disappearance of calcium carbonate in bursitis with calcification by means of the same diathermy.
4. The increase in basal metabolism under autocondensation.
5. The increase in urinary solids, hitherto deficient, by autocondensation, and at times by the sinusoidal current.
6. The deposition of calcium salts in rickets.
7. The influence of actinic energy not only on the hemoglobin content of the blood but also on the number of red cells, and not infrequently on their differential count.
8. The increase in the alkali reserve under the administration of radiant heat."

As has been said many times, physical therapy is not a separate cult. It is a part of the bone and sinew of medicine and surgery. It is their adjunct to ensure functional and physical restoration.

In industrial medicine, physical therapy properly used, by shortening the time of disability, results in appreciable economic gain to the patient, to the industry, and to the community at large. The Rehabilitation Clinic in Syracuse, New York, is an example of this. Here, in fifty-one months, four hundred and seventy-four cases have shown a saving of $163,000 over the evaluation of the state board. Properly applied physical therapy—Aye, there is the rub! In many places, insurance companies have bitterly complained of bills rendered for alleged physical therapy which has not shortened the time of disability and which has greatly augmented the industrial financial loss.

Too often a physician, a possessor of one or two pieces of apparatus, his only knowledge a salesman's say so, boldly holds forth as a specialist in physical therapy. As a warning against this the Council on Physical Therapy has said: "A physician who has installed a diathermy machine or an ultra-violet ray generator can do good in carefully selected cases with one of these methods. He is not, however, fully equipped to render physical therapeutics. As a rule, it is the careful combination of several

physical agencies that gets the best results." This combination of physical measures is the keynote to success. It frequently is not enough to prescribe one or more of the various forms of electrical manifestations and omit from the prescription massage, muscle reëducation, or hydrotherapy. Such treatment is not properly applied physical therapy. Because of this, though it is a short-sighted policy, many insurance companies, and some state industrial boards have fixed fees for physical therapeutic treatment which are too low to cover the actual expense of an adequate treatment. What is the answer? It lies in constituting a preferred class. In some states the medical board has the right, through the authority invested in it, to examine and register persons desiring to practice any limited branch or branches of medicine and surgery, and to establish rules and regulations governing such limited practice. In this way the incompetents would be weeded out and those thus registered as specialists in physical therapy would soon prove that the laborer was worthy of his hire by the more rapid return of the industrially incapacitated to gainful occupations.

In the limited time at my disposal it will be possible to consider in detail only a few of the ramifications of this subject.

The value of electrosurgery in properly selected cases is so well known that discussion of it here would be repetition.

There is, however, one electrical manifestation which is in popular favor, both lay and medical—the ultra-violet ray. This is pregnant with possibilities for good and for evil. The newspaper and periodical write-ups have created an intense public interest. It is natural that the fond parent should desire that his offspring, in the months deficient in ultra-violet radiation, should enjoy Florida sunshine, while attending school in northern climes. Some of the manufacturers of apparatus have been quick to enter this profitable field, and though ostensibly under the

guise of selling or renting apparatus only under a physician's prescription, yet in some instances they have conducted a house to house canvass of likely prospects. Clubs also have been shown the income to be derived by the installation of sun rooms. The bulk of this is not under any adequate medical supervision. If the apparatus sold delivered a spectrum which was analagous to the solar, the risk would be less—though even here untoward results have been observed. The solar ultra-violet rays, the so-called near or vital, are those to which the human organism has been tuned to respond. In the most popular form of ultra-violet irradiation, the mercury vapor arc, the ultra-violet radiation consists of these longer wave lengths plus others much shorter than those to which the body is accustomed. Under such conditions the medical supervision should be careful and constant. Because of untoward results, the French Government recently has placed the use of ultra-violet under the laws of practice of medicine act and only physicians can legally employ it. In some cases a markedly decreased resistance of the hæmapoietic organs ensues. In other cases ultra-violet radiation has acted like a protein shock bringing about a prolonged negative phase. After the ingestion of certain articles of food or hypnotics of the barbital group, sickness may ensue. Hence, under such conditions the sale of ultra-violet apparatus to the laity or clubs should be restricted to that in which the spectrum is limited to that of the solar, that is not lower than 2900 Angström units. It has been claimed that by repeated skin irritation cutaneous cancer may be produced. I have yet to find an authenticated case of this. On the other hand, certain skins markedly susceptible to the sun's rays may develop an intractible chronic eczema.

The reverse of this picture is a brighter story. Take for example tuberculous conditions, as lupus vulgaris, tuberculosis of the bones (surgical tuberculosis), tuberculous adenitis, tuberculous peritonitis, tuberculous kidney, and, less successful, intestinal tuberculosis. In these conditions

the results are suprisingly good. *Pulmonary tuberculosis.* Here, ultra-violet rightly used may be a valuable adjunct. Too long an exposure over too great a surface is generally followed by disastrous results.

Probably the Vitamin D evolved from the action of ultra-violet on the cholesterol of the skin accounts in part for the increased calcium and phosphorus content of the blood and it affords at least a plausible explanation for the results obtained in rickets, spasmophilia, and certain deficiency diseases.

Indolent wounds, discharging sinuses and *cutaneous ulcers* respond at times miraculously. In diseases of the skin the results are variable. Certain types of eczema do well; others poorly; and still others are made worse. *Psoriatic* patches generally disappear but their reappearance may be looked for any time thereafter. *Sycosis, tinea,* and *impetigo contagiosa* respond quickly to this treatment. In *herpes zoster,* if used early, it is almost a specific and apparently there is less likelihood of disagreeable after paresthesias. *Pruritus,* causative factor if found of course removed, is generally relieved. In *acne vulgaris* the results are as a rule excellent though at times it may stimulate a growth of hair on the face. If this phenomenon would occur consistently on areas where it is so often deficient, the manufacturers of ultra-violet apparatus would be obliged to build additions to their factories. In *alopecia areata* there is frequently re-growth of hair. The *Journal of the American Medical Association* has reported a number of cases of *purpura hæmorrhagica* and *hæmorrhagic* disease of the newborn which have been so markedly benefited by general ultra-violet irradiation as to warrant its employment in such pathology. Ultra-violet seems to be valuable as an adjunct to the modern dietetic treatment of pernicious anemia. At the Boston City Hospital cases seemingly at a standstill have promptly improved when general ultra-violet irradiation was added to the previous treatment. In blood pressure the administration of ultra-violet gives variable results, the tendency being towards

reduction. This may be due in part to dilatation of the cutaneous capillaries.

Recent laboratory studies apparently show:

1. That short exposures to ultra-violet increases the hemoglobin content and number of red cells to a slight extent in normal blood. After cessation of treatment there is a rapid return to normal.
2. That short exposures also have the same effect in cases in which the red blood cells and hemoglobin are lower than normal. They tend to remain normal.
3. That after a single exposure the blood platelet count is increased, successive daily doses increases the count progressively. The immediate effect is a sharp drop followed by a marked rise above normal.
4. That there is an initial drop of the lymphocytes as a constant reaction to all ultra-violet exposure whether in single or successive daily doses. If a massive dose is given a leukopenia of several days' duration results. This is suggestive in view of the tendency towards self-treatment.
5. That if the dose is not too great, ultra-violet wave lengths shorter than 3000 Angström units produce, following the initial drop, an increase in the lymphocytes.
6. That wave lengths shorter than 2750 Angström units have no effect on the polymorphonuclears, while wave lengths between 2750 and 3200 Angström units will produce a sharp rise which attains its maximum in five hours. This increase lasts for several days.

All laboratory and clinical observations show that there is an optimum dose of ultra-violet and that an excessive dose may be harmful. At present there is no exact method of standardizing dosage, though for the average practitioner the lithopone unit of Dr. Janet Clark will be found to be the most satisfactory.

The relative merits of heliotherapy, the carbon arc, and the mercury vapor arc are still *sub judice*. It is probable that specific indications will be found for each. Factors

foreign to the carbon and mercury arc enter into the use of heliotherapy for here we have the effects of variation of temperature, frequently of altitude, the metabolic whip of the breeze on the naked body, and generally a complete change of environment and dietetic regime.

Diathermy is a close second to the ultra-violet in the professional mind. This form of high frequency electricity has the property, because of internal tissue resistance, of inducing heat within the tissues themselves. The hot water bottle, the electric heating pad, electric light applicator, the hot soak, and the whirlpool bath have a more or less superficial effect due to the limited heat conduction power of the skin. It is true that vaso-dilatation does take place. The blood stream becomes warm to a greater or less degree and in that way may act reflexly through stimulation of the central heat centers, and the blood from the deeper lying structures is determined to the skin, thus producing depletion if there is deep seated engorgement. On the other hand, with diathermy, the skin is less of a factor. Its resistance and that of the tissues in the path of the current causes a deeper seated heat to be generated than by any method with which we are now conversant. A short time ago pretty geometrical figures were drawn showing that given two unequal bases the intersection of diagonal lines drawn from the ends of one base to the other would be the point of greatest heat intensity. This is not strictly true though the point of greatest heat will be nearer the smaller electrode. In living tissue we have another story, for here the circulation of the blood dissipates an appreciable amount of the heat and the varying degrees of resistance of the tissues encountered also play their part. Some experiments seemed to prove that the heat penetration was relatively slight, though far superior to any other method, but more careful and scientific laboratory check-ups with more sensitive apparatus show that in the living subject tissues are heated to considerable depths. In general, diathermy may be used in any condition in which heat will give relief. Its main contraindications are where by dilatation of blood vessels

hemorrhage may ensue or where there is pus without drainage. As its main component is heat, and if proper attention is paid to technic, there is less chance of harming the patient through an ill-advised prescription than there is in the use of ultra-violet or the other subdivisions of physical therapy. In combination with ultra-violet it is useful in certain types of asthma and bronchitis. In subdeltoid bursitis, with or without calcification, added to galvanism, massage and manipulation it ordinarily will afford speedy relief.

Static electricity is an anathema to many, and a Mecca to others. To one who is able to steer a middle course it will be of great service. Too often, though, in the form of the static wave current it is prescribed for conditions in which massage and muscle training would be infinitely better.

Only the pathology commonly met with should be considered in a summary like this. Especial attention should also be paid to any procedure or practice which if persisted in might harm the patient or the fair name of physical therapy.

The promiscuous rental of radium to physicians is an example of the latter. This may be dangerous both to the physician and the patient. A surgeon who would not undertake an unusual operation without infinite attention to detail will without previous experience or taking the time to learn the fundamental principle governing the action of this radio-active substance, fill out a blank giving the site, size and character of the lesion and attempt to follow the instruction of the radium specialist of the company. This specialist, who may not be a physician, will give directions for the treatment of a patient he has never seen, to a man who knows little of the risks, reactions and dangers of such treatment. The result may be good, or it may lead to injurious or disastrous consequences.

Fractures. According to insurance companies' statistics, and a report of a special committee of the American

College of Surgeons, fractures as a rule are rather badly handled. A part of this is frankly bad surgery or poor reduction; but most serious of all is the loss of function after union has taken place. This is generally due to too long immobilization and the non-use of physical measures of treatment. If physical therapy is started early, stiffness and ankylosis are generally prevented. Trophic changes are minimized and the end result is not only bony cure but functional restoration.

Stiff and Painful Knees. This affliction, occurring after middle age, may or may not show any demonstrable pathology. Diathermy and galvanism will generally, if there is no mechanical reason to prevent it, give for many months a symptomatic cure. The pain will be relieved and the range of motion will be greatly increased, even to normal. There are apt to be exacerbations but these in turn yield to a similar treatment.

Pneumonia. Discounting all therapeutic claims made for the use of diathermy in pneumonia the one fact that in the majority of cases pain is relieved and the patient has more sleep, justifies its use as a routine measure. In hospitals where this is employed as a routine measure the amount of narcotics or anodynes has been decreased more than one-half. The fear that the treatment will disturb the patient is not well founded for generally he will ask if it is not time for the next application.

Non-Union or Delayed Union. Here again given adequate fixation, diathermy or ultra-violet irradiation, or both, will ensure the formation of callus and if other suitable physical therapeutic measures be added functional restoration will result. Without fixation the chance of success is relatively small.

Arthritis. The etiological factors should be sought for and if found, eliminated. Frequently, under physical measures of treatment, symptomatic cures of many months in duration will be effected. The prescription will vary. In any event it should be a composite one selected from

the following: Hydrotherapy, heat, including the electric light bath cabinet, diathermy, galvanism, static electricity, massage carefully guarded as to time and character, and gentle manipulation.

There are many omissions. Much that is of real value has been left out. Some of the abuses of physical therapy both in its use and its non-use have not been mentioned. My only aim has been to show that:

1. Physical therapeutic measures of treatment are being placed on a sound and rational basis.
2. Physical therapy has secured civil and governmental recognition as one of the triad of medicine, surgery and physical therapy.
3. Nearly all the mysticism and much of the empiricism formerly inherent in physical therapy has been removed.
4. The wide-spread laboratory and scientific clinical investigations which are now under way will soon make it a more exact art.
5. In many conditions complete physical and functional restoration can only be secured by the use of physical therapeutics as an adjunct to standard medical and surgical procedures.
6. To secure this teamwork is essential.
7. Physical therapy should be practised on a broad and comprehensive basis.
8. If it is so practised not only will the time of disability be shortened but many who otherwise would have been hopeless cripples will be returned to financial independence.

Because of the standing physical therapy has already attained it will not in the future be necessary to close in the words of John Wesley, the author of the first book in English on electricity, who, in 1760, said: "Before I conclude I would beg one thing—it is that none of them would condemn they know not what; that they would hear the cause before they pronounce its sentence."

ABSTRACTS OF PAPERS DELIVERED AT SECTION MEETINGS

Section of Historical and Cultural Medicine, March 9, 1928

THE ORIGIN AND ANTIQUITY OF MAN IN AMERICA

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It affords me greater pleasure than I can express to speak in the Academy of Medicine. When I studied medicine in New York the Academy, and especially its Library, in the old building in 41st Street, was of the utmost value to me and I found so much there that I shall never cease to be grateful.

As the subject of my lecture this evening, perhaps I might have thought of something more medical, and yet I think that before we finish you will see that even in this subject, which seems so distant from the more direct medical interests, there is a good deal of human psychology, and here and there perhaps even a bit of psychiatry.

The question of origin and antiquity of the American Indian began to be a problem from the moment Columbus reached these shores. He, of course, believed to have reached only the eastern part of the Asiatic continent and that the people he encountered were the people of India, wherefore they came to be called Indians.

Later, when it became known that America was a separate continent, and that the people of America and all the larger islands about it were a race or body of people of their own, there developed much speculation in Europe as well as among those who already had reached this Continent, as to who these people were, and from whence they came. Unfortunately there was at that time but one recourse in all questions of this sort, which was the scriptures; so people began to search the scriptures, but they could find no reference to any such race as the Americans.

And so many reached the pernicious conclusion that, as long as these people were not mentioned in the scriptures where every human variety was accounted for, they could not be equivalent with other men, they were something less; which created a prejudice the results of which were sad for the Indian. The notion in fact became so mischievous that one of the Popes finally was obliged to issue a special "Bull" proclaiming that the Indians were human beings, equivalent to other more primitive peoples of the earth, and should be treated as such.

After this and as the Indian became better known, speculation took a different turn, and writers began to identify the Indian with various peoples of antiquity, particularly those near the seas and known to have been navigators. These identifications ranged from the Irish and Welsh to the Ethiopians, Phoenicians, Egyptians, etc. But the most prevalent idea was that the Indians were the descendants of the ten lost tribes of Israel.

The initial scientific inquiry into the matter begins with one of the fathers of the country, Thomas Jefferson, and with his learned contemporary, Professor Benjamin Smith Barton. From this time the studies advance by leaps and bounds and begin gradually to assume more solid foundations, this especially through the early government exploring expeditions, Clark and Lewis, Fremont, the Union Pacific surveys, and others—expeditions which brought back a great deal of new knowledge and also many specimens. Then followed the classic works of Samuel G. Morton, on Indian crania, by Squier and Davis on the Mounds, and by Schoolcraft and his associates on the living Indians.

The greatest impetus, however, to the study of the antiquity of the American Indian was given in the late fifties and especially the sixties and seventies of last century, by the discoveries of ancient man in Europe.

The first important human remains in Europe were discovered in 1848 on the Rock of Gibraltar; but they, up to

1863, remained hidden in the local museum and were unknown. In 1856, however, in Western Germany, near Düsseldorf, in the Neander gorge, workmen found a remarkable skeleton, which at first by even such authorities as Rudolf Virchow was taken to represent some human monstrosity. But some scientists of the time, including Huxley, and soon all the rest of the scientific world, became convinced that the remains represented an early and very noteworthy human type, hitherto unknown. As the result of this discovery the old skull of Gibraltar was resuscitated, cleaned of the rock in which it was enclosed, and was shown to be another striking example of ancient humanity; more than that, it proved to be the first known ancient lady—it was the skull of a female.

All this acted like a wild fire on the scientific circles, with the results that from that time on all through western and later on central and southern Europe, northern Africa, and surrounding parts, investigations of early man went intensively ahead; as a result of which there are such treasures, both in the cultural line and in the line of actual skeletal remains of ancient man, accumulated in the museums and institutions of Europe, that the whole subject has long since passed from the field of conjecture to that of ever growing and clearing knowledge of human evolution and human antiquity.

It was natural and logical that these discoveries in Europe should arouse interest in the question of antiquity of man in America, and it was not long before men like Thomas Wilson, who as a Consul in Switzerland became acquainted with the archeological remains of early man in Europe, began to believe to see similar conditions in America; from which it was only a step to declaring that ancient man or his cultural remains were also found in America.

In 1879, U. D. Whitney, a leading geologist and paleontologist of California, reports a remarkable "ancient" skull, known as the Calevaras cranium, believed to have been found in the tertiary deposits of a deep mine. But

later studies by Holmes, Merriam and others could confirm no such derivation of the specimen, and the skull finally was adjudged as that of a fairly recent California Indian.

There followed other findings of "ancient" man, a number especially on the western coast of Florida, where human remains were discovered actually in rock. Joseph Leidy, luckily, was able almost at once to subject these finds to careful study, and reached the rather simple conclusions that the remains, while petrified and actually enclosed in some cases in stone, were nevertheless of no extreme age and could be attributed to the Indian.

At the same time much attention was being given to the Mound Builders, and many reached the opinion that these, presenting so much higher culture than the "wild" Indians, must have been people of a different race, and probably of greater antiquity. This subsequently culminated in the remarkable belief of the geologist and paleontologist Winchell, of Minnesota, that the Mound Builders represented two distinct "dynasties," both of which were of Quaternary time. Since then, it is almost needless to say, these views were found to be wholly erroneous.

Still later, remarkable developments began to take place in Argentina. There a paleontologist of renown, Florentino Ameghino, began to report one after another human remains, more or less fossilized and found under such conditions, stratigraphic and as to associations with extinct animals, as seemed to indicate great antiquity. Before long he reached a remarkable classification of these remains, attributing some of them to very early men, and still others to prehuman creatures or human precursors. The latter he divided eventually into four classes which he called, respectively, the Prothomo, Di-, Tri-, and Tetraprothomo; and he believed to have found even lower creatures which have given rise to human evolution on this continent itself. His culminating hypothesis was that man evolved in South America, that over land connections he

reached Africa and then Asia, Europe, and finally North America.

Florentino Ameghino died, but his brother, Carlos, also a paleontologist, went him one better, announcing soon after the death of Florentino the discovery of a human-made arrowpoint in the femur of a Miocene Toxodon. The Miocene is so far back that not a trace of human beings or even of the human precursors can possibly be imagined in that period; yet there was the femur of the Toxodon with the arrowpoint sticking in it, and so it was concluded that man, and that already an arrow-using man, was contemporary with the animal.

Still later various additional finds of supposedly ancient man were made in North America, some cultural, in some instances merely charcoal, in others parts of human bones; and in each instance there were conditions which seemed to point to geological antiquity. The most notable of these finds were those of Trenton, New Jersey.

The subject was assuming such importance that the Smithsonian Institution made it a point to follow up these different discoveries. First was begun a critical study of the different older reports in North America, which was followed by that of the sites themselves and of the specimens; and in 1910 similar work was extended also to South America.

The earliest Smithsonian studies of this nature were those of William H. Holmes, in association with whom were men such as Salisbury and Chamberlain. One of the richest discussions of the whole subject took place in Detroit in 1897 at a meeting of the American Association, where the foremost workers interested in this problem were present, and discussed the whole question of man's antiquity in America. That meeting showed, perhaps for the first time, the marked and curious division between the views of the geologists and paleontologists favoring the acceptance of man's antiquity in America, and the more

critical men of these branches, with the experienced archeologists and anthropologists, who opposed such an acceptance on the evidence presented. This division in views and opinions has lasted ever since, exists to-day, and it can hardly be expected to be settled within this generation, for it is involved with certain potent human psychological peculiarities.

The study of the skeletal remains, believed at one time or another to be of geological antiquity, in both North and South America, was carried on since 1898 by the speaker, first in New York, at the invitation of Prof. F. W. Putnam, and then in Washington. A rule was made not merely to peruse the publications and see wherever possible the actual people concerned in these finds, but to go, where still possible, to the sites where the discoveries were made, and to see the originals. And wherever there was a geological question involved, men were taken along who had all the confidence of other workers in their line, to clear up the problems as far as it might be possible.

After finishing in this manner the studies of the North American claims it was found, briefly, that none of these were of such substance and weight that they could generally and confidently be accepted as establishing ancient man. The results have been published in Bulletins 33 and 66 of the Bureau of American Ethnology, and in other places.

In 1910 a trip was made, in association with one of the deans of American geology, Bailey Willis, to Argentina. The Argentinian specimens up to that moment and even after the North American specimens were found not to have the necessary qualifications, seemed to offer something more hopeful. And I may say right here that the American archeologists and anthropologists approached this question always with the hope that something of this nature, that is real ancient man, may be discovered in America, for if he were discovered here it would give a great impetus and support to all our investigations.

We therefore went with considerable hope, and upon reaching Argentina were given all possible facilities. In fact Florentino Ameghino went for some weeks himself with us, along the isolated barren sand dunes of the coast. Much ground was covered, from Buenos Aires to Patagonia and from the east coast to the Andes. The objects reported as ancient were examined, the conditions studied —and we could not corroborate the antiquity. The Diprothomo, for instance, was found to be merely the frontal portion of an ordinary human skull. The Tetraprothomo femur, the femur of some extinct cat. The "tierras cocidas," or burnt earth, supposed to be of ancient human origin, proved volcanic.

As to associations with extinct forms, we found such associations. There were carapaces of Glyptodones and other fossil animal bones, unquestionably ancient, and near them and with them were arrow points, stone knives and other artifacts, even stone anvils upon which these things were made. But the association was evidently not that of antiquity.

Many of the human bones reported as ancient were petrified more or less; but we found conditions over large areas such that a bone could hardly escape becoming more or less mineralized in comparatively short time. There were, for instance, bones of beef and sheep, thrown out from a hut of Italians a few years ago, already heavier than normal and assuming more or less the aspect of fossilization.

In short, after an earnest piece of work, both Bailey Willis and myself could reach but one conclusion, and that was that the entire series of reports of early man in South America was based on conceptions that could not be sustained. And thus tumbled down the elaborate structure of ancient man also in that part of the American continent. For a complete report on the South American finds see Bull. 52, B. A. E.

Since, other geologists and paleontologists have reported "ancient" human remains from different parts of the

United States, especially from Florida. One of the most notable of these examples was the so-called Vero find, announced in 1916 by the geologist of the state at that time, Dr. Sellards. (See Bull. 66, B. A. E.). In that case six or seven scientists from different branches were invited to examine the position. Unfortunately, the invitation came only after the initial finds were extracted and after rains and other conditions removed the original section of the bank of the canal in which the remains were found. Nevertheless it was possible to make a large new exposure and this, together with the study of the discovered human bones, stone implements and fragments of pottery, left no ground for the supposed great age of the remains.

Still more recently finds of relics of ancient man are being reported by paleontologists again from Florida and also from Oklahoma, western Texas and New Mexico. Some of these finds are evidently of such a nature that they will necessitate prolonged and careful studies before any final judgment can be passed upon them. Others have already been discredited, most recently the one in Oklahoma (see *Science*, Feb. 10, 1928, p. 160).

And so we are laboring in trying to find and establish ancient man in America, with one failure or uncertainty after another. Even the best cases have left so far little more than a taste of disappointment.

How deceptive these matters are sometimes, and how they will delude even educated men, may be illustrated by a few examples.

A remarkable instance relates to the Rancho de La Brea human remains, in California. Not far from Los Angeles are found, in the semi-desert country, peculiar sinks or funnels in the earth, containing asphalt or pitch. In this asphalt are in places many bones of Quaternary animals, constituting veritable paleontological treasures.

In one of these pits of La Brea, in 1914, were found, among various animal bones, portions of a human skeleton, including the skull and lower jaw. And the lower jaw

looked very unusual. For the first time among our finds, there was a jaw that seemed to have a receding chin, as the more ancient jaws of Europe; and the skull itself seemed to be peculiar. Thanks especially to Dr. John C. Merriam, the whole sbuject received a careful expert scrutiny. And the result was but another disappointment. The skeleton, at first announced as very ancient, proved to be simply that of an Indian woman of a type well represented in California; and the chin had been rubbed off through friction in the funnel.

In 1924, during the construction of an outfall sewer for the city of Los Angeles, at some distance outside of the city, the bones of six human skeletons were found at a depth of 19 to 23 feet. Shortly after the first bones were found, the newspapers all over the country were surging with the news of the discovery, with more or less unbounded statements as to the antiquity of the specimens. They were older than the Neanderthal man, older even than the Pithecanthropus.

At that time the Smithsonian could not send anybody to see the discovery; but Dr. John C. Merriam, the head of the Carnegie Institution, asked Professor Stock of California, an able geologist, to go and examine into the conditions. And Professor Stock did an excellent piece of work. The skeletal remains were collected and the main parts brought to Washington, where they were examined and compared; and the site itself received all possible attention. The results were reported the same year by Stock and Merriam to the Academy of Sciences, at Washington. The gist of the report was that both the layers in which the bones were found and the bones themselves, were of but moderate, non-geological antiquity (see *Science*, July 4, 1924, pp. 1-5).

And so could be mentioned case after case. There is much romance in these things. I could entertain you the whole evening with enumeration of such cases alone, and some of the mistakes made sometimes are almost unbelievable. In one case one of our own colleagues, who has found

some human skulls and bones in California, reported them as of considerable antiquity, and as one of the evidences of this antiquity was given the fact that there were seven inches of separation between the two jaws of one of the skulls, which would be impossible in man of to-day.

In another recent case a paleontologist at a scientific meeting declared, showing a crushed skull from Florida—though by no means so crushed that there could not be recognized many of its parts and its modern type—that the crushing of it was an additional evidence of its antiquity, for it could hardly have been produced otherwise than by a mastodon stepping upon the ground containing the specimen.

So there we are! We see two lines of scientific men, all in earnest and bent on finding the truth, yet opposing each other diametrically on a problem that can not be settled by argumentation or beliefs, but only by as ample and unequivocal evidence as settled it in other countries. But here we strike again a lot of human psychology and even some psychiatry.

The remedy is, first of all, a more adequate preparation, experience and critique of those who would engage in these investigations. They are not for the amateurs, or the beginners, or those who take them up merely as a side hobby. I believe in the future every worker who undertakes to examine into the question of the antiquity of man in America should have due experience as well as other qualifications, and should be thoroughly acquainted with the conditions of human antiquity in the rest of the world, especially Europe. A worker without such qualifications is so thoroughly handicapped that he is in danger, whether on one side or the other, of making an error.

As it is, the anthropologists are accused that they cannot agree with the geologists because they have not studied geology. And the anthropologists retort, on the other hand, that they have studied all they need in the laboratories of Nature, and that the geologists and paleontologists are quite innocent of anthropology. And so it goes on.

The geologists and paleontologists tell us that stratification means a great deal. We tell them that on the basis of our experience, not book knowledge, stratification may sometimes be produced in almost no time, and cannot alone be decisive in many a given case. Most of the geologists and paleontologists place a great weight on fossilization of specimens. To the anthropologist fossilization is essentially a question of geophysics and geochemistry, and not of chronology. The geologists and paleontologists lay great store on association of extinct forms with the human; for many of them the dogma of "same horizon or stratum, same age" is the rule of rules; while the anthropologist gives little weight to such associations unless they are corroborated by all other evidence, knowing that man differs radically from animals through the introduction of his dead from 2½ to 7 feet deep into the earth, regardless of the age of the deposits he penetrates or their contents. The average paleontologist believes implicitly that a bone of an extinct animal dates whatever human artifacts may be found with it; the archeologist says it is far more probable, in many cases, that the artifact dates the bone, or has no time relation with it whatsoever. And there are other points of difference. It would seem logical and relatively easy for the two sides to come together and settle these differences, but—psychology prevents.

The human remains in the Old World teach, in fact, a great deal. In the first place it is seen that, as the Ice Age advances, there are definite correlations between time and human culture, as well as physique. The farther back the time, the more primitive are both the physique of the man and also his culture, until somewhere in the earliest third of the glacial period there is reached a stage where even experts are not able to say, in many cases, whether an artifact is already an implement made by a human being or simply a work of nature; or whether the osseous remains are already fully human.

The conditions in Europe teach further that as the latest glaciation advanced man lived to a large extent everywhere, where he was present, in caves and rock-shelters.

Not one favorable cave or shelter for human habitation in old France, Belgium, Moravia, the foothills of the Pyrenees, and elsewhere, that has not been the place of habitation of some ancient man.

Furthermore, in these caves and the terraces or refuse heaps outside of them, as well as in his sites in the open air, the ancient man of Europe, Africa, etc., has left the amplest kind of evidence of his presence. He has left there his implements, in places literally by the thousands. In one of the Aurignacian sites lately discovered and where less than one acre of the many covered by the remains has been excavated, already more than 300,000 of objects worked by man, have been recovered. A cave on the Island of Jersey, not yet half excavated, has yielded to date over 20,000 stone implements and other objects of human industry. At La Quina, in southern central France, the specimens recovered are measured already by barrels and the site is still far from exhausted.

So everywhere, wherever ancient man lived, in caves or in open air, he has left not a single implement or what not, as appears in the American finds, but large numbers, such numbers that already they collectively clog some of the museums in Europe. Even our museums already have large collections.

In addition, early man, wherever he lived, lived essentially on the animals of his time. He was in the main a hunter. He trapped the great animals in holes in the ground, and killed the smaller animals in traps or in other manner. In the course of time he consumed great numbers of these prehistoric, now extinct, animals; and he left their bones as a testimony of his presence. These bones, the charcoal of his fires, besides the chips of the stones he worked and the implements that he lost or gave up or buried when he left, are there, and they show and teach plainly what the man was when he lived, in a large measure what he did and what animals he lived upon. Many of the animal bones show even where and how he cut the sinews or broke the bone to get at the marrow, or broke

the skull to get at the brain. There is a plain great book, in which it is easy to read for any intelligent man, not even a scientist.

Where are any such things in America? Where are the habitations and refuse accumulations of our old rivers and caves? Where are the implements, the bones of the animals upon which these old men have fed? The paleontologists say there have not yet been examined sufficient numbers of caves or sites. Let us not be deceived. The collective number of caves already examined by American paleontologists and geologists themselves, besides archeologists, already reaches a very respectable figure; while the open air sites and mounds explored reach into the thousands. Yet to this day not a single case of anything such as in Europe.

Where is the explanation of all this? What is the matter? If ancient man existed in America he could not have been represented by a few individuals who left a single or a few arrowpoints here and there in association with a single or a few fossil bones. In cities everywhere there are constantly carried on dredgings, excavations for foundations, water pipes, sewers. In the country there are innumerable railroad and road cuts. Many such have been watched or are supervised by educated men some of whom, at least, would report finds of odd things. But to no result.

A whole series of eminent men in American archeology, from Cyrus Thomas to F. W. Putnam, and Holmes, Fewkes, Hough, Mills, Moorehead, have given a large part of their lives to excavations and study of the American remains, but without finding a trace of anything earlier than the Indian.

The National Museum in Washington has in its collections over 12,000 old American crania from many parts of the two continents, and in the rest of the American and European Museums there are probably as many more; yet not a single specimen that could be accepted as of any real geological age.

This shows what tremendous difficulties are confronted in trying to establish man's geological antiquity in the New World, and these difficulties can not be done away with by mere semblances, or opinions, or assertions.

There is always also the great back-ground question, as to how could ancient man have reached America. It is known that up to the maximum of the last glaciation, man in Europe had hard times to sustain himself, reached no high numbers, and left even in that continent many of the available parts unoccupied. He may have spread thinly as far as the Caucasus, as far perhaps even as the Central parts of Asia or much of Africa. But this spread, according to all the sound indications there are, was a very slow spread and of a difficult nature, for there were as yet no means of transportation over waters, and no domesticated animals. Up to the middle Aurignacian period man had such imperfect and so few implements and so many other handicaps, that he cannot possibly be conceived as having spread over the ten or more thousands of miles over Asia towards the approachable parts of America, opposite Alaska. Before he could reach that far and pass over, he must have occupied all the more easily reached and better suited parts of Asia, and he must have developed means of effective coast navigation. All of which is inconsistent with glacial man as known from Europe.

These are the difficulties that confront the anthropologist in his quest for ancient man in America. Yet he keeps his mind open. It would not be worthy of a true scientist to be prejudiced, or even to give up hope while even the slightest chance of new and more satisfactory discoveries may remain. At the same time, the American student must be cautioned from ever accepting what cannot be proven to his full satisfaction.

One of the collateral important lines of studies, in these connections, was to find how much there is in America of a racial differentiation. And there was found remarkably little, which is inconsistent with any great antiquity. There are, it is true, sub-types of the American Indians, a number

of them; but the differences between them are less than the differences between, for instance, the Italian and the Scandinavian in Europe. Why should man not have differentiated more in America if here since before these post-glacial differences were realized in Europe?

Anthropologists have also gone to Asia and scoured the lands there for vestiges of the race that has given the American Indian. And they found them, found them widely, and in some cases still so pure, so typical, that they could not be distinguished, physically and even in behavior, from the Indian.

One evening in Mongolia, at the end of the day, I was sitting down, a bit tired. Suddenly a Mongol was heard coming towards our house, singing. The song was Mongolian. I could not understand the words. But the intonation, the character of the song, was so utterly Indian that I had to turn and shake myself to find where I actually was, to appreciate that I was not among some of my old friends of the Mexican Sierras.

I must conclude. I merely want to add a second constructive suggestion. The first related to a more adequate and critical training of the scientific progeny, which will be called upon to grapple with the problems here dealt with when the older generation is gone. For the second—I regard as the most important step the creation, by the American scientific bodies that are most concerned, of a flying group or Committee, composed of the most experienced men in all the branches, which would be notified of every promising find and which would go wherever circumstances warranted to examine on the spot, and before things were disturbed, all that was to be examined.

DISCUSSION

DR. BRUNO OETTEKING: The problem which has been so ably presented to us to-night by my learned friend and colleague, Dr. Hrdlicka of Washington, you have seen is

one of enormous magnitude. With the science of physical anthropology a number of other sciences are implicated such as geology, zoology and paleontology, archeology, and so forth. However, when it comes to the real nucleus of the problem, then we have to deal with the physical man, man in all his appearances and in all his different surroundings and which the scientist of to-day calls the phenotype of man.

Man himself, as Dr. Hrdlicka has also explained, occurs in the line of evolution in certain successive forms. We are generally agreed to recognize two different types of mankind, one very primitive and one more advanced. The primitive type of mankind is *Homo primigenius,* as represented by the Neandertal race with a number of varieties, which has been found over a fair stretch of the surface of the earth, particularly, however, in Europe, France, Spain, Croatia, and of late in Africa and Asia Minor.

This primitive type of mankind, the *Homo primigenius,* is of a very crude appearance and so are his cultural achievements, as important as his place in the history of evolution ever may be. But Nature herself always progresses from the crude to the more refined, and from the simple to the more complex. The different finds to which I have just referred as having been made in various localities and in different parts of the world have all more or less the same common characters of primitivity. These are recognized, for instance, as a very low and sloping forehead, supraorbital ridges, the so-called occipital torus, a very broad nose, projecting jaws, the general crudeness of the skeletal bones but particularly of the femur which is the strongest bone of the entire skeletal system of man and of which we have a number of excellent specimens, such as the Neandertal femur, and a number of others of the same type.

The following more refined type of man which occurs toward the end of the glaciations is a direct forerunner of the present or recent type of man. The recent type of man was called *Homo sapiens,* the wise man or the man

who knows, by the great classificator Linnaeus, in consideration of the other forms of highest vertebrate development, which as a mammalian order he called primates.

Now the most ancient representative of this more modern, more recent human type is the man of Cro-Magnon, and it is from this form that the present types of man known as racial differentiations are derived.

The methods by which the physical anthropologist distinguishes and differentiates these various phylogenetically distinct types of mankind up to the recent racial differentiations are the methods of physical anthropology and it may perhaps be of interest to give a brief description of them.

From a general viewpoint we can distinguish between descriptive and metrical methods. By the descriptive methods, for instance, the color of the skin, the hair, the iris, etc., are distinguished. This is done preferably by means of a color table. The intensity of coloring or pigmentation is due to the amount of pigment in the skin which is of organic origin and which in the body household serves as a protection against the injurious rays of the spectrum, in particular the ultra-violet.

The color is not the only criterion in racial description. There is also the form of the hair, the form of other integumental appendages like the eyelids, lips, the female breast, and a number of other diagnostics. Descriptive methods can be practiced to an excellent degree of exactness by a conscientious and discriminating observer. However, the still more exact means of physical research is given by the metrical methods which account for the size of the body and its parts, and afford their proportional treatment. One of these measurements, or rather a computation from two of them, is quite popular and known as the cephalic or cranial index. The two principal diameters of the human head or skull, *i.e.*, their length and breadth, are involved in it and the index derived from them is indicative of short- or longheadedness, and the intermediary forms.

It is an interesting observation that from the evolutional angle the primitive type of hominoid appearance and their close relatives, the anthropoid apes, are longheaded or better longskulled. This holds true for the greater number of them, for instance, the Neandertal, and even the still more primitive *Pithecanthropus erectus,* which was also mentioned to-night. Among a greater number of specimens of the Neandertal type, however, recovered by Gorjanovic-Kramberger at Krapina in Croatia, the occurrence of a shortheaded variety could be stated by that author.

In the following more advanced type of Cro-Magnon we have a similar occurrence, *i.e.,* the man of Cro-Magnon was longheaded. I believe it was the German anatomist Schaaffhausen who spoke of a disharmonious type of skull in characterisation of the cranial configuration of the Cro-Magnon. Thus, while the face of the more advanced type of skull is more in correlation, with the brain case, *i.e.,* a narrow face in connection with a long and narrow brain-case, the Cro-Magnon has a broad face in connection with a narrow brain-case which from the viewpoint of configuration gives rise to a disharmonious cranial appearance.

The longheaded variety of mankind is still the predominating one in certain parts of the earth. In the more primitive types, in fact, in the lowest ethnological layers, like the Australians, Melanisians and certain Negro tribes there obtains pronounced dolichocephaly. However, there are also dolichocephalic varieties in different parts of Europe, while on the whole, a tendency toward a rounder head seems to be the general trend in human evolution. There are whole provinces, whole domains in the Old World where longheadedness has been replaced by broad- or shortheadedness. First, for instance, in the greater part of Central Europe, proceeding northward even as far as Scandinavia, and the northwestern regions of Europe.

Speaking of methods used in physical anthropologic research, the descriptive and metrical ones have been mentioned. These, however, do not exhaust the anthropolo-

gist's ways and means, and in order to round out the picture the physiologic methods of blood reaction and blood agglutination should at last be enumerated. By the former the degree of blood relationship between man and closely related primate forms is estimated by the degree of serum reaction between any two types, while by the blood agglutinative reaction racial relationship is recorded.

What has all this got to do with the American aborigines? Well, exactly the same as what we may apply to other racial types anywhere in the world. We use the same methods of anthropological research upon the American aborigines, the descriptive as well as the metrical ones. And I may say that the physical anthropologists in America are fully aware of the task before them, since their specific research represents a problem which is still in its primary stages of solution. There cannot be any doubt, however, that by concentrated effort we will be able sometime fully to solve the problem connected with the derivation and racial affinities of the American Indian. It is by the exemplary efforts of the principal speaker of the evening that valuable advances in that direction have already been made.

MR. N. C. NELSON: It seems necessary to say a word about the general status of the evening's discussion. As matters stand, you will all, I fear, go away with the idea that the problem before us is hopelessly many-sided and difficult. Such a conclusion is certainly warranted from what you have heard. Nevertheless, please bear in mind that each one of the speakers, if he could be heard through, has a fairly consistent story to tell; and be assured, moreover, that if the discussion could go on for another hour or two you would detect indications of agreement such as do not now seem possible.

Dr. Hrdlicka without doubt has given our topic more serious study than any other man living. By comparison I feel as if I had myself done next to nothing. To date I

have to my credit only five or six field investigations bearing directly on the problem, and so far nothing decisive or even promising has come of these studies. In addition I have reviewed the literature bearing on a long list of earlier discoveries. The result is that I am personally not at all enthusiastic about the outlook for demonstrating the great antiquity of man in America.

In general, Dr. Hrdlicka has confined his remarks to skeletal remains. He has specialized, of course, on that type of data. On the other hand, I have specialized on cultural data and I might properly bring the discussion to a close by merely saying that on the whole my conclusions correspond pretty closely to his own. By this I mean that practically all the recorded archeological discoveries in America for which geologic antiquity has been claimed (including the one at Folsom, New Mexico, described for us by Dr. Brown) agree in showing skeletal types or artifact types which, in terms of our knowledge of somatic and cultural developments for the world as a whole, are both essentially modern. In other words, our oldest artifact remains are typically Neolithic, while of the true Paleolithic culture complex, recognizable over the greater portion of the Old World, we have in the New World not yet positively isolated a single trace.

It is a temptation, however, to add a few words in an effort to show you the seemingly paradoxical nature of our problem. I have made a hasty analysis of probably most of the discoveries and alleged discoveries relating to the geologic antiquity of man in America. All told I have on record no less than 164 finds. Seventy-four of these were accompanied by fossil remains, comprising more than twenty extinct animal species. One hundred and four consisted of tools and weapons, while the other sixty disclosed human skeletons. Twenty-four finds were made in caves and one hundred and twenty-four in open geologic exposures such as the one Dr. Brown has shown us. Sporadic and more or less accidental as these interesting discoveries have been, they spread over about a century of time

and they were made by people in all walks of life, including men of scientific training and experience. Geographically the finds range from southern Canada to Patagonia and geologically they are said to extend from post-Glacial times back to the Miocene epoch. Now the Miocene, according to recently revised time reckonings, dates back approximately ten million years. Consequently, the investigator who may be disposed to accept at face value our 164 American discoveries recited above is confronted with two rather difficult propositions. For one thing, he is compelled to grant that an advanced type of culture—the type generally called Neolithic—such as first appears in Egypt and Mesopotamia about 20,000 years ago, in America commenced ten millions of years earlier. For the other, he is forced to admit that this Neolithic culture persisted in America for ten million years without appreciable modification! No student of early human developments at all familiar with the archeological facts presented by the Old World can possibly entertain any such view with reference to the New World.

On the other hand, it is claimed that our human precursors existed in western Europe since before the Miocene epoch. We have recently obtained proofs that already in Pleistocene times man ranged from England south through Europe to Rhodesia and South Africa; southeast through Palestine and Java to Australia, and eastward to the vicinity of Peking in China. Under these circumstances it becomes hazardous to affirm that man was not also present on the American continent during the Pleistocene. The fact that the skeletons found here are of modern stamp does not necessarily disprove their great antiquity. There are those to-day who believe that when our early ancestors separated themselves from the rest of the animals by starting on a cultural career, they at the same time stepped out of the great stream of forces which make for organic evolution. Man since that day has been under no necessity to undergo radical bodily modifications by way of specialization or adaptation to new environments. When occasion required he changed, not his body, but his

culture—a thing much more quickly effected. Now Sir Arthur Keith, of the Royal College of Surgeons in London, believes that he finds, among the fossil remains of the Old World, evidence of a modern human type dating back to and beyond the times of the primitive Neanderthal men. At any rate, he is certain that *Homo sapiens* is of very ancient origin and that therefore our modern-looking American skeletal finds may well have a right to the geologic age claimed for some of them.

But if man's physique has been comparatively stable since his culture began, the same cannot be said for culture itself. Culture is an exceedingly plastic phenomenon, especially in its higher stages. Moreover, it has never been known to burst into full bloom without previous cultivation or transplantation from without. When therefore we seek to explain the genesis of our American Indian accomplishments we have no choice but to look to the Old World, and when we do we find there the counterparts of many of the basic American elements. Thus one might name almost offhand nearly one hundred simple tools and weapons which are common to America and at least portions of the rest of the world. And when we seek for the forerunners of these implements we find them not in the New World but in the Old.

In conclusion I may add that for my own part I am sitting on the fence with respect to the whole question of the antiquity of man in America. True, I lean now this way and now that; but I have scarcely any doubt at present as to which way I shall ultimately jump. To tell the truth I am no longer greatly concerned about this phase of our American problem. In the meantime, should it be of interest to anyone, I would say in closing that the Neolithic culture may very well be of really considerable duration on this continent. Thus, if we may judge at all from the apparent date of origin and the rate of development of this culture phase in different parts of the Old World, it seems entirely possible for man to have sojourned in America for a period of anywhere from 7,000 to 15,000 years.

DISCUSSION

Recent Finds Relating to Prehistoric Man in America

Barnum Brown: In my hand (displaying a box containing stone artifacts) I hold the answer to the antiquity of man in America. *It is simply a question of interpretation.*

Those who have reviewed the subject extensively will, I think, agree with Dr. Hrdlicka that prehistoric man was an emigrant to America; that he probably came from Asia; that he was Mongoloid; and that the first emigrants did not arrive in America until after man had reached a development superior to the Neanderthal race.

Probably there have been several driblets of emigration, but as to when the first emigrants arrived in America we differ.

If we hypothecate a route by which people reached America from Asia at one time, there is no reason for supposing this route did not persist over a great period of time and permit more than one emigration.

Taking into consideration the physical development of the less progressive peoples of to-day, especially some of the Mongoloids, the ancestral stock of the present day Indians probably existed in America long before historic time. Since his first appearance in America man has used stone implements, and if we could find the skeletal remains of the very first emigrants they probably would not differ greatly from those of primitive Mongoloids of to-day.

Dr. Hrdlicka makes all of his comparisons with finds made in Europe, but such comparisons are not entirely warranted, for conditions in Europe and America are not the same. In Europe where most of the remains of prehistoric man have been found, there are great areas of eroded limestones with caves and natural shelters. In America we are dealing chiefly with plains, or forested sections, where caves and shelters do not exist. In the Southwest, however, where there are sandstone caves and

shelters, we find evidence of prehistoric man, but of a comparatively recent date.

Since early Pleistocene time, among fossil mammals many species have undergone little or no change, and some cannot be distinguished from those living to-day. They constitute as much as 25 to 40 per cent. Not a single genus of mammals has been evolved during the whole of the Pleistocene period. During this time some races of men probably continued for long periods with little physical change. I am sorry the short time does not permit me to discuss more than one of the several occurrences that many feel establish the presence of early man in America.

One of the cultural phases I am exhibiting here to-night —artifacts that were found associated with the remains of an extinct species of bison found in New Mexico, during the past two years by an expedition from the Colorado Museum of Natural History. Many artifacts similar in form are to be found in various museum collections in the United States, but they have not before been recognized as representing a cultural phase. Now that we can associate them definitely with an extinct animal, of considerable antiquity, and the surrounding conditions preclude their having been made by historic men, they are definitely recognized as a cultural phase.

These artifacts are of distinct Laurelate form, excavated or fluted on both sides, and are retouched to a finer degree than any heretofore discovered, rivalling, if not surpassing, the workmanship of any stone culture in Europe or Egypt. A distinctive feature is the form of the blade, hollowed on the sides with the widest part anterior to the middle. Unlike the triangular form of modern arrowpoints, which wedge from the moment they enter a skin until they penetrate beyond the barbs, this type of point makes the greatest incision at the initial impact, thus, with the same impetus, penetrating farther into a resisting body than the triangular point. These artifacts are probably darts, antedating the bow and arrow, and if they are recognized as distinctive, may be designated as the "Folsom culture."

Folsom, New Mexico, is about 60 miles east of Raton, in the northern part of the State. Much of the country is covered with lava 30 to 130 feet in depth overlying Niobrara shales. In places the lava plateau has been eroded. At this particular point the lava has been eroded forming a horseshoe-shaped basin covering an area of approximately five square miles. The walls of the basin are vertical and we think the flow was of Pliocene age. There are numerous extinct craters in this vicinity. Subsequent to the erosion of this basin, one of the craters again became active throwing a secondary flow across the two prongs of the horseshoe, thus closing it at the lower end. This flow is 40 feet in depth. Undoubtedly it dammed up the natural drainage and a lake was probably formed in the lower part of the basin.

If I have interpreted the occurrence correctly a group of prehistoric bison were surrounded on the upper border of this lake by a number of prehistoric men and several bison were killed—probably a meat-kill. Eight skeletons, or partial skeletons, so far have been recovered and test holes sunk last year reveal bones underlying an area equal in extent to that already excavated, so that we may assume there were at least eighteen or twenty bison entombed at this place. The site of the quarry is on a level with the top of the secondary lava flow.

Subsequently the impounded water has broken through the secondary lava flow and carried part of it away, the basin drainage now passing at the face of the flow. During recent times a ravine has cut through the strata that overlaid the skeletons and exposed some of the bones. In 1925 Mr. Fred J. Howarth and Carl Schwachheim discovered these bones. The find was reported to the Colorado Museum of Natural History and Mr. J. D. Figgins, Director of the Museum, investigated the finds. Mr. Schwachheim, with assistance, excavated there in 1926-27, taking out remains of eight bison skeletons—seven bulls and one cow. The cow skeleton was articulated and the seven bulls somewhat disassociated.

One of the bulls is now mounted in the Colorado Museum of Natural History, and it shows many characters that distinguish it from other species. It has recently been named *Bison taylori*. The animal was relatively higher in the flank than the living bison, bones extremely massive with the highest spines of the dorsal vertebrae only 19 inches in height, whereas, those of a large living bison are 23 inches in height.

The principal characters, however, are found in the enormous skull with its wide-spreading horns, great width between the eyes and extremely narrow muzzle.

In the first year's work an artifact was found near one of the ribs, and another on the same bone level, but not distinctly associated with the ribs. On resuming work the second year two other points were found on the dump where they had been uncovered by winter rains. This material had been thrown out during the previous year's excavation—during the last of the quarry work, which was at the bottom of the quarry. While in Utah, in September, 1927, I received word from Mr. Figgins that a fifth point had been found. We went immediately to the excavation and after making a careful examination of the geology and surrounding circumstances, I had the satisfaction of removing the last five inches of sediment that covered the fifth point.

The sediment that covered these skeletons is restratified Niobrara shale eight feet thick. A careful examination of this fifth point *in situ*, precludes any possibility of it having been introduced subsequent to the burial of the skeletal remains. This point laid within 2 inches of a rib and parallel to it. Ribs and arrow were taken up together in a block and are exhibited in Denver.

In this part of New Mexico the average yearly rain fall is 14 inches. The quarry site is less than three-quarters of a mile from the abrupt lava cliff that forms the border of the basin, and the material that forms this deposit must have accumulated by freshets at intervals during a long

period of time. Chamberlin and Salisbury have determined that it takes three thousand years to denude one foot of surface in the Mississippi valley. Now if we reverse this process and apply the same standard of comparison to deposition, it would have taken twenty-four thousand years for this deposit to accumulate over the bison remains. I am convinced, however, that this is only an estimate of the time involved. On account of the sheltered situation the estimate is probably too low.

In conclusion: After finding this definite type of fluted, or excavated, point associated with an extinct bison we have searched many collections and find fourteen of similar shape and peculiarity in the Andover collection; one hundred and eighty in the Ohio State Archaeological and Historical Society collection; several in the Kentucky collection and several in the American Museum collection—all from central and western states. So far as known these were surface finds, but previous to the Folsom discovery no one was able to distinguish them as a cultural phase, and it is quite possible that there are other, at present, unrecognized cultures represented in collections of stone artifacts.

Section of Neurology and Psychiatry, May 8, 1928

CASE REPORT ILLUSTRATING THE EARLY DIAGNOSTIC SIGNIFICANCE OF THE ENCEPHALOGRAM

E. D. Friedman

P. B., farmhand; born in Russia; 36 years old; single.

Patient was first admitted to the neurological service of Bellevue Hospital on July 13, 1927. His chief complaints were headache, pain over the entire right side, dizziness especially following forced defecation, "lights before his eyes," pain behind the ears, especially in the morning, and generalized convulsions. The latter had occurred at irregular intervals for one and a half years prior to his admission.

Family history was negative.

Previous history: Served in the Russian army during the War, and was gassed. Contracted gonorrhoea in 1917, and suffered from right sided pleurisy in 1918. Tonsillectomy was performed in 1923.

Physical examination revealed good pupillary reactions, normal fundi and no visual field defects. There was a slight right facial weakness with widening of the palpebral fissure. The abdominals were diminished on the right, and the right plantar response was equivocal. The right corneal reflex was diminished. Pin-prick was better perceived on the left and the patient complained of subjective pain on the right. There were no other sensory changes. No aphasia.

Spinal fluid was clear, colorless and aside from the presence of 54 cells per c.mm. revealed no abnormalities. Spinal and blood Wassermann tests proved negative. The cerebrospinal fluid pressure reading was 420 mm.

The diagnosis of a focal lesion in the left hemisphere was made; and encephalography was suggested in order to determine the nature of the lesion.

Lumbar insufflation of air was carried out on August 7, 1927. The encephalogram revealed a shifting of the ventricular system to the right. The anterior horn of the left ventricle was narrowed. The third ventricle was obliquely displaced to the right and the anterior horn of the right lateral ventricle was somewhat dilated. The encephalographic appearance was that of left cerebral neoplasm.

The general medical status proved negative. X-ray examination of the chest revealed no abnormalities. The urine showed no abnormal findings aside from a faint trace of albumin. Blood pressure readings were 132/84.

Patient left the hospital at his own request.

He was readmitted on December 28, 1927. His chief complaints at this time were recurring fits, loss of strength

and impairment of "memory." He still spoke of bright colors before his eyes just before the convulsive seizures, which recurred about once a week. They seemed to be provoked by loud noises (acoustico-motor reaction), and were followed by persistent dull pain over the entire right side. The members of his family observed a notable impairment of his memory and a marked slowing up of his cerebration. He did not seem to understand spoken words as well as he previously did. This was especially true of conversation in English (his more recently acquired language).

Physical examination revealed no impairment of the sense of smell on either side. Visual acuity was 20/20 in each eye but perimetric examination now showed notching of the upper temporal field on the right and the upper nasal field on the left. The blind spots were enlarged in both eyes. The fundi showed the presence of papilledema, more pronounced on the left. There was definite weakness of the right side of the face, also evident on mimetic innervation. The tongue deviated somewhat to the right. There was a slight motor weakness of the right side. The deep reflexes were more active and the abdominals diminished on the right. The right plantar response was equivocal. Temporal anemia was now present. Neither agraphia nor alexia could be demonstrated. It was observed that the convulsive episodes began with twitching of the right arm and that they were followed by weakness of the right side and by the appearance of a definite Babinski sign on the right. There were no uncinate phenomena or dream states.

The clinical diagnosis of left temporo-sphenoidal neoplasm was made and operation was advised.

Exploratory craniotomy was carried out on February 15, 1928. Under local anesthesia, an osteoplastic flap was turned down on the left side, exposing the parietal and temporal lobes.

The dura was tense and did not pulsate. A needle was inserted into the posterior portion of the temporal lobe and

about 10 c.c. of dark brown semi-fluid material was evacuated. A second needle was introduced into the anterior portion of the temporal lobe with similar results.

The dura was incised; the brain appeared yellow and the convolutions flattened. The cortex in the temporal region was incised. A large necrotic mass was palpated in the depths and about 15 c.c. of this was removed.

The dura was left open. The bone flap was cut away slightly at its lower margin to allow a small decompression far from the motor cortex, and the flap was returned to position very loosely to permit an increase in intracranial volume.

Post-operative course: Patient has done fairly well since the operation. The convulsions have not returned. There have been no striking changes in his neural status except for the recession of the papilledema.

Comment: The history and the meagre findings in the beginning were suggestive of focal disease in the left hemisphere. The encephalogram gave the clue to the nature of the pathological process long before the advent of papilledema.

The location of the neoplasm, medially and deep, probably accounted for the thalamic pain on the right.

THE SURGICAL TREATMENT OF GLOSSOPHARYNGEAL NEURALGIA WITH REMARKS ON THE DISTRIBUTION OF THE GLOSSOPHARYNGEAL NERVE

Byron Stookey

Dr. Byron Stookey presented a patient on whom he had done an intracranial dorsal root section of the glossopharyngeal nerve. Complete relief of all pain followed immediately after dorsal root section with no recurrence of pain.

Thus far only eighteen cases of true glossopharyngeal neuralgia have been described in the literature. This case is the first to be done at the Neurological Institute. The patient complained of paroxysmal sudden severe pain beginning in the region of the tonsil and base of the tongue, and referred to the front of the ear and region of the maxillo-mandibular joint. The paroxysms were brought on by swallowing or by touching the base of the tongue or tonsil. The pain of glossopharyngeal neuralgia is so typical that once seen it is readily distinguished from trigeminal neuralgia.

A single incision suboccipital craniotomy was done and the nerve sectioned as it left the brain stem without injury to any of the adjacent nerves, particular care being taken to cut only the funiculus of the glossopharyngeal root so that a pure glossopharyngeal paralysis would result. Thus an almost unprecedented opportunity was presented for a study of the function and distribution of the ninth nerve. Isolated paralysis of this nerve has been unusually rare and thus far only two previous isolated dorsal root sections of the ninth nerve have been done.

Dr. Stookey found clinically that the glossopharyngeal nerve had no independent representation in the cutaneous innervation of the ear. That the naso-pharynx, Eustachian tube, soft palate, anterior and posterior pillars of the fauces, tonsil, base of the tongue and dorsal wall of the pharynx, receive their sensory innervation from the ninth nerve. The motor innervation was found to be the palatopharyngeus and palatoglossus muscles with a fiber to the superior constrictor. The latter muscle receives, however, its major innervation from the vagus.

Section of Obstetrics and Gynecology, May 22, 1928

PUERPERAL MORBIDITY AND MORTALITY

P. Brooke Bland

Philadelphia

Puerperal morbidity and mortality are the chief concern of the conscientious obstetrician and it is recognized that pregnancy has its perils.

In the septic days of Mariceau, Holmes and Semmelweiss septic mortality was high compared to that of 2.7 per 1000 live births to-day.

In this country more than one expectant mother out of every 200 dies in labor or shortly thereafter. The United States Children's Bureau statistics indicate that nearly 25,000 mothers die annually in this country. In 1915 the death rate from all puerperal causes was 6.1 per 1000 live births and in 1905 the rate was 6.5 per 1000. Compared with figures available from 20 other countries the United States had the highest mortality of all in 1924-25.

Since morbidity precedes mortality, it is obvious that it is wholly impossible to improve our mortality statistics unless we first improve our morbidity statistics. Far too many women are being sacrificed on the altar of maternity. The subject of the high rate of maternal mortality has been given concern in other countries and the Chief Medical Officer of Great Britain, Sir George Newman has urged the importance of better obstetrical care in many of his reports although the rate in England was 4.1 per 1000 live births in 1926.

The causes of mortality are chiefly septicemia, toxemia and hemorrhage. Septic infection claims more than one third of the victims and there is lack of prenatal care, indiscriminate manipulation and injudicious implemental delivery.

It is believed that 75 per cent. of the cases of sepsis are associated with repeated internal examination and various

injuries. Repeated examinations, injudicious manipulation and operative procedure favor infection.

I would recommend the restriction of all local examinations to the abdominal wall except in complicated emergencies during labor.

The mortality from Cesarean sections runs as high as 88 per 1000 and of 100 deaths following this method of delivery 30 per cent. are caused by septicemia. There is a decided reaction in this country against the use of instruments in normal cases to hasten delivery as the increased risk is more generally recognized.

Hemorrhage is the third factor of importance in causing mortalilty. The causes of hemorrhage are equally divided between a normally placed placenta and placenta previa. Hemorrhage from premature separation of the placenta is almost invariably an expression of the toxemia of pregnancy. The dangers of unavoidable hemorrhage may be largely reduced if diagnosis is made promptly and active interference is performed. Delay promotes an increased percentage of maternal deaths.

Recommendations made at the annual meeting of the British Medical Association in 1905 to have a morbidity standard that all postpartum cases that had a temperature of 100 or more should be considered morbid. Similar recommendations were made at the Congress on Puerperal Fever in Strassburg in 1923.

I believe that a standard of morbidity in this country is very desirable and I make an earnest plea for a conventional standard of maternal morbidity in the United States.

The various standards so far suggested are wide apart:

1. In the Boston Lying-In Hospital, Newell's standard is 100.4 and his percentage of morbidity is recorded as 4.5 per cent.

2. The standard followed in the Carney Hospital is also 100.4 but Phaneuf says his percentage is 10 per cent.

3. George Gray Ward in the Women's Hospital, New York, regards every patient as morbid in whom the temperature reaches 100.6. During the year 1922 with 507 deliveries the morbidity was 1.38 per cent.

4. The standard in the Lying-In Hospital, New York, is 100.4 or more on two successive occasions exclusive of the first twenty-four hours. In 1927 with 4,871 deliveries there were 201 patients or 4 per cent. classed as morbid.

5. Harold Bailey in his service in the Bellevue Hospital employs a standard of 100.4 for two days excluding the first day. His total obstetric morbidity is recorded as 9.6 per cent. The morbidity rate for the operative cases is reported as 21.7 per cent. The mortality rate covering a period of five years with 5,520 indoor and outdoor deliveries was .59 per cent., a most commendable record.

Dr. Bailey properly believes that the exclusion of both vaginal and rectal examinations during labor, reduces the morbidity and in 58 per cent. of the 4,396 indoor patients, no vaginal or rectal examinations were made.

6. In the Long Island College Hospital, Polak follows the 100.4 standard. He reports a morbidity of 5.8 per cent.

7. In the Philadelphia Lying-In Hospital, the standard is a temperature of 100, on two occasions after the first day. For the past three years the morbidity has consistently ranged around 29 per cent.

8. The standard followed in the Department of Obstetrics, Jefferson Medical College Hospital, is a temperature of 100.4 on two successive or on two different days after the first twenty-four hours. Under this system, our percentage of morbidity is rarely below 30 per cent. The percentage for 1927 was 31 per cent.

9. In St. Margarets Hospital, Pittsburgh, Titus uses the standard followed in the Rotunda Hospital, Dublin, namely 100. He reports a morbidity of 20 per cent.

10. Williams, in the Johns Hopkins Maternity, employs a standard of 100.4 at any time during the puerperium except the twenty-four hours immediately following delivery. His morbidity is reported as 13.56 per cent. in white and 25.21 per cent. in colored patients.

11. In the Chicago Lying-In Hospital a patient is regarded as morbid in whom the temperature reaches 100 on a single occasion from the moment of delivery until the day of dismissal from the hospital. Under this rigorous standard, or as DeLee says, "the strictest of all standards," only 1881 or 11 per cent. of the 17,275 patients confined from 1918 to 1925 inclusive, were, he claims, registered as morbid. By estimating the morbidity according to the standards of the British Medical Association, only 801 or 4.1 per cent. of the patients could be placed in the morbid category.

12. Findley, of Omaha, Nebraska, employs the standard of 100.4. He believes that fully 33 per cent. of his patients reach that point by the third day, postpartum. His morbidity percentage, he says, he does not regard as excessive, but "An honest to goodness acknowledgment of facts."

From the recital of the foregoing, it is observed there is no marked variation in the standard of morbidity employed by the institutions named, ranging from 100 to 100.6. The majority seemingly favor a standard of 100.4. The variation in the morbidity percentage is the conspicuous feature of this meagre study, ranging from a little over 1 per cent. to beyond 30 per cent. With a standard precisely the same, differing only in the fraction of a degree, it is difficult to reconcile the vast discrepancy in the figures reported.

It is proposed in Great Britain to set up a representative maternal morbidity committee by the Minister of Health and if a conventional standard could be applied in this country, it would serve for promoting scientific study, for statistical comparison, for scientific uniformity of classification, and for fostering better obstetrical work in general.

There should also be a standardization of the principal causes of morbidity and these should be divided into the intrinsic and extrinsic causes. The intrinsic causes should include the following:

1. All forms of septic infection, both pelvic and systemic.
2. Traumatic or wound infection.
3. So-called lochial retention.
4. Mammary infection.
5. Phlebitis, always an expression of infection.

We should remember in endeavoring to prevent maternal mortality that the most skillful obstetrician of all is Nature herself. John Mowbray (1724) said that it was one of the duties of the "man midwife" to "watch the patient during pregnancy so as to prevent all preternatural disorders."

John W. Ballantyne in 1901 recommended pre-maternity clinics and secured pre-maternity beds in the Royal Maternity Hospital at Edinburgh and before his death in 1923, there were over 600 pre-maternity clinics in England and thousands of others throughout the world.

In New York City in a study made of 8,743 mothers who received pre-maternity nursing under the direction of the Maternity Center and the Henry Street Settlement, the mortality from eclampsia was reduced to about one-third of the usual percentage. Ninety-five per cent. of these patients with toxic albuminuria earlier went to full term.

The maternal death rate amongst these women from all puerperal causes except septicemia, was 2.06 per 1000 live births compared with 2.84 in Manhattan Borough as a whole.

The reports of the Obstetric Department from the City Hospital, Louisville, Huddersfield, England, Toronto General, and Antenatal Clinic have demonstrated that pre-maternity care largely reduces the death rate from puerperal morbidity.

The profession finally as a whole must be brought to realize that pre-maternity care and a scrupulous aseptic regime are peerless essentials in the general obstetric plan. Injudicious operative obstetrics, or to designate it by its proper descriptive name, meddlesome midwifery, is not in accord with sound obstetric philosophy.

To neglect the former and practice the latter will one day have to be accounted for to an educated and comprehending public, for adequate supervision of every patient during pregnancy and assiduous care during parturition are the factors of safety on which motherhood chiefly must depend.

BOOK REVIEW

A New History of Surgery*

For now many years, a history of surgery of moderate compass, at once readable and reliable, has been a desideratum. Gurlt's three volumes of 1898, checked by Sudhoffs' massive contribution on mediæval surgery (1914-18), constitute the standard reference works and store-houses of information up to the end of the 16th century. Few busy surgeons, however, have time to read them in the original, and here, Sir Clifford Allbutt's delightful St. Louis address (1905), covering much the same ground, has been a primer of immense service to English-speaking physicians. Billings' survey, in the Dennis "System" (1895), was the most accurate, informing and critical account of the whole subject in English, but as being constructed along the old Haeser-Daremberg plan of paragraphing a long succession of names, *more bibliographico,* it is a bit dry and disappointing, hence is more an infallible librarian's reference-manual, for checking up on facts and dates, than a going or inspiring narrative, to be read and reached out for with gusto. Apart from Allbutt's little book, which is quite as precious and symphonic in its way as (say) Stopford Brooke's "Primer of English Literature," the nearest thing we have to a moving, informing narrative is perhaps George Fischer's "Surgery a Hundred Years Ago" (1876), which was Englished by C. H. von Klein in the *Journal of the American Medical Association* during 1897-8 (vols. xxviii-xxx *passim*). The volume before us, by Dr. Walter von Brunn, professor of medical history at Rostock, was, as the talented author states in his preface, written to be read rather than to be shelved for reference. A book of this kind obviously fills a long-felt want. Illustrated with no less than 317 effective cuts, its deliberate object is to set off the recent mechanisation of surgery, which has occasioned so many timely growls and heartfelt groans on the part of our German colleagues. The

*W. von Brunn: *Kurze Geschichte der Chirurgie,* iv (1 l.) 339 pp. 8°. Berlin, J. Springer, 1928.

surgeon's calling, like the dentist's, does happen to be, in part, mechanical, whence, as von Brunn very rightly affirms, his readiest cultural offset is to be found in the history of his profession; meaning by "culture," not the namby-pamby, pigeon-livered, Bunthorne concept of the man in the street, but "the knowledge of the best that has been thought and said in the world," the knowledge which is power. For this reason also, as our author truly says, no one could (or should) hope to write a meaningful history of surgery who is not an actual practitioner of the art. The tyro reveals his inability to think surgically at once. To convey the essential *raison d'être* of surgery, the historian must answer three leading questions: Why is surgery? What is surgery? Whence and how came surgery? The best answer to the first query (always transcendental) is perhaps contained in an observation of Rebecca West—"To those who fall and hurt themselves, one runs with comfort; by those who lie dangerously stricken by a disease one sits and waits," which takes us at once into the rationale of prehistoric man's thorn-punctured abscesses, fish-toothed saws and decompressive trephinings against blindness, at the same time stressing the Roman (Themison) view of internal medicine as "a meditation upon death." Surgery has thus always preceded internal medicine, as being the most aggressive and effective of all arms of primitive therapy, a purposeful attempt to "meet death coming" and cheat impending doom. How came it? Initially, from the primitive intuition that "relief from suffering is an attainable goal," which, in mechanics, is Maupertuis' Principle of Least Action, or as the old Scotch lady said, getting rid of a temptation by yielding to it; for even a monkey will pick at a biting insect or an offending thorn. Later came simple observation of the effects of puncture, incision, boring (trephining), sawing or even of letting well enough alone. From his initial sentence—"the oldest of healing methods is surgery"—our author winds into these questions with intelligence and ability. Getting rid of foreign bodies and staunching hemorrhage was the probable starting point. Then came splinting of fractures

(the dog on three legs), trephining with chipped flints and the fashioning of knives and saws. Throughout these antique phases, the scheme of illustration is so exhaustive and complete that it would make an almost intelligible "movie" of the subject. The neat groups of primitive and ancient surgical instruments, assembled and photographed by Meyer Steineg and Sudhoff, are all included, and one misses nothing except such items as the Venetian bleeding glasses of the 17th-18th centuries, the wound-suckers of the 18th century, certain statues, inset placques or medals commemorating famous surgeons, effective pictures of recent military phases or modern oil paintings of surgical operations. Striking indeed are the Finnish bath (p. 30), the ex-voto objects (44-52), the Etruscan bridge-work (81), the kits of Roman instruments (106-107), Andry's crooked tree as an analogue of the strapped scoliotic spine (247) and the idyllic photo (286) of Billroth at St. Gilgen (*"Das ist ein Abend wie ich keinen sah"*). The atrocities which pass for Behring (256) and Mikulicz (287) might easily be replaced by better photos in a later edition. The narrative throughout is flowing and going, there is but little cluttering up of the story with factual references (dates), which have been otherwise well taken care of in von Brunn's introduction to the Kirschmann and Nordmann System (*Die Chirurgie,* Berlin, 1926, I, 1-76). Our colleague sustained very grave mutilating injuries in the late war, which have compelled him to relinquish the actual practice of his profession for the teaching of its history. His book is cordially recommended to American surgeons as an attractive and reliable introduction to the subject. Merely to glance at the pictures is to understand why the ancient Teutonic name for surgery was *die Heilkunde* (the healing art) and why great soldiers like Frederick and Napoleon sniffed at internal medicine (a poor thing in their day), but loyally upheld the surgeon.

F. H. Garrison.

COMMITTEE ON PUBLIC HEALTH RELATIONS

MATERNAL MORTALITY

A Report to the Committee on Public Health Relations

Harold Bailey

We have many times heard the statement that the United States stands eighteenth on the list of civilized nations as regards maternal mortality. If this is true it is disgraceful and should spur us on in our efforts to reduce this death rate. On March 27, 1927, according to a report in the *New York Times,* Sir George Newman of the Ministry of Health of England, at his lecture at Yale University, made the statement that the "present maternal mortality of America is the highest among civilized nations of the earth, and twice that of Great Britain." This statement is clearly an exaggeration of the facts as I showed in a table which compared the rates of Great Britain with those of the United States for 1921 and 1922 and which was published in the *American Journal of Obstetrics and Gynecology,* December 1926. I present this table again with the two succeeding years added and while there is very little difference in the crude rates of the United States and Great Britain, the adjusted rate is slightly in favor of the latter country. It must have been that Sir George Newman intended to refer to England when he compared us with Great Britain for England alone has a rate that averages around 3.9 per 1000.

Maternal Mortality in England, Scotland, Wales and the United States

(From U. S. Census Report)

		All Puerperal Causes				Puerperal Septicemia			
	Rate	1921	1922	1923	1924	1921	1922	1923	1924
Eng., Scot. & Wales	Adjusted	6.9	6.0	6.2	5.7	3.2	2.2	2.6	1.8
	Crude	8.1	7.2	6.9	6.6	3.5	2.5	2.7	2.2
United States	Adjusted	6.7	6.6	6.5	6.2	2.6	2.3	2.4	2.3
	Crude	6.6	6.5	6.4	6.1	2.6	2.3	2.4	2.3

In regard to our place on the list of nations, the crude rate for the United States for 1924 was 6.1 and the following countries were as high or higher: Canada 6.6, Denmark, Norway and Sweden 6.4, Ireland 7.9, Germany 6.3 and Poland 6.1. The adjusted rates for the same year put us at 6.2 and all other countries as low or lower excepting Ireland which was 8.3. Admitting that our rate is altogether too high for a country with the facilities from the standpoint of wealth and of interest in maternal care, let us inquire into the causes.

What are the Causes of the High Maternal Death Rate?

1. *The Mixture of Races in this Country.* In the first place it seems to be due in part to our racial mixture. Race influences the size and the general conformation of the pelvis, as for instance, Indians and Negroes have small round pelves while the Germanic races have large pelves with open outlets. The Nordic countries and Germany have very little pelvic deformity so that operative interference in labor is lessened.

2. *The Negro Women.* Our registration area in 1915 consisted of ten states and the District of Columbia. Today it consists of thirty-nine states and the District of Columbia and includes practically all the Southern states. Therefore, a considerable number of states with a high Negro death rate enter into the total death rate for the United States. The death rate of Negro women during 1924 was 11.8 as compared to 6.1 per 1000 for the white women. This is partly due to the Negroes' entire disregard of hygiene and ordinary care and partly to imperfect registration of births, *i.e.*, a high percentage of miscarriages are not recorded because the United States bases its rate on live births. The live births and stillbirths recorded together would give a much more favorable comparison provided the registration was complete. Finally it is quite evident that the lack of medical care or even the care of a trained midwife at the time of delivery is probably the

largest factor in the high Negro death rate. I will speak later of the Negro women in New York City and what may be done with concentrated care.

3. *Postabortal Sepsis.* Another factor increases the general rate and that is the deaths from postabortal sepsis which are listed under maternal mortality. Abortions in New York State are one in every four births and one-third are criminal, as estimated by the late Dr. Eichel of the New York State Department of Health. In other cities the abortions are much more frequent. In St. Louis they are estimated 1 in 2.3 births; in Minneapolis, Adair gives the figure as one in three plus births. If it is advisable to record these deaths from sepsis following abortion, they should be classified as postabortal sepsis and not included in the maternal mortality rate; otherwise we have women who have never borne children dying and being classified under the maternal death rate.

4. *Operative Interference.* One-third of our maternal deaths are due to puerperal sepsis and about 26 per cent. to toxemia with or without convulsions. At least 40 per cent. of the fatalities from sepsis follow operative interference. In the United States there is undoubtedly too much operative interference. We have teachers in the great centers in this country encouraging vicious teaching. DeLee urges his so-called "prophylactic forceps operation" which is quite an extensive surgical procedure and should never be attempted by the general practitioner; and Dr. Potter practices his so-called "prophylactic version" which with him means a podalic version in every case possible. Then there is the high incidence of Cesarean section which in Potter's cases is 7 per cent. and in some localities reaches a figure as high as 9 per cent. Where an attempt is made to keep down the number of Cesarean sections the incidence is low. On my Bellevue Hospital service it is 1.7 per cent. for a five year period, 1921-1926.

In the State of New York one out of every five women die following Cesarean section. In Massachusetts the deaths are one in seven and the fatality for the operation

is 8.8 per cent. Many of these deaths, of course, are due to imperfect selection of the cases, Cesarean being done for eclampsia and on cases late in labor and which are already infected. As an example of how high the Cesarean death rate can run, one hospital in New Orleans reported 117 cases of abdominal section with 12 deaths due to peritonitis, 2 to sepsis, 2 to the operation and 25 to the disease for which the operation was performed, the total rate for Cesareans being 35 per cent. It may be added that in the same issue of the same journal, a paper from Chicago showed a death rate from the low transperitoneal Cesarean of less than one per cent.

5. *Failure to Hospitalize Cases.* There is another point that applies very largely throughout the country. The general practitioner believes that he is credited by his patients with being able to care for women in labor and therefore when operative intervention is necessary, he hesitates to send his patients to a hospital and seldom consults a specialist. The result is a serious operation performed in the home with the nearest doctor available acting as consultant and assistant.

What is the Obstetrician Doing About the Maternal Mortality?

1. *Time and Better Teaching.* Proper teaching should repeatedly stress the need of conservatism in the operative treatment of women in labor. Teachers are demanding more time from the medical colleges for teaching obstetrics and they are attempting to teach more practical obstetrics, using less of the didactic methods.

2. *Demonstrations.* The obstetrician is demonstrating that complete care will reduce the death rate. These demonstrations are made in correlation with nursing organizations like the Maternity Center Association and the Henry Street Settlement in New York and also in many clinics. In Dr. Beck's Long Island College Clinic, he was able to reduce the mortality for one year to 2.5 per 1000. At the Berwind Maternity Clinic in Harlem we were able

over a five year period to reduce the death rate of white women to 2.6 and that of the colored women to 3.9, or a total of 3.2 per 1000. Both these clinics—one in South Brooklyn and one in Harlem—are used for teaching fourth year students who are in residence at the clinics. The city rate for the same years was from 4.2 to 5.0 per 1000. The Maternity Center Association reports that in 1999 so-called "closed cases" there were 5 maternal deaths or a rate of 2.5 per 1000. Thus it has been definitely shown in the City of New York that the rate may be lowered about one-half by prenatal, natal and postnatal care.

The Sheppard-Towner Act

The Federal Government combined with the State Governments is making efforts to decrease the mortality by teaching prenatal care and by lecturing to practitioners in urban centers, and where the teaching has been carried on there has been a lowered death rate from toxemia. In States like Virginia where there is a real need for trained midwives to care for the Negro women, no effort is being made to train the midwives, for in Virginia they are following an impossible ideal represented by the slogan "a doctor to attend every woman in labor." The work carried out under the Sheppard-Towner Act is frequently directed by women who have not the practical knowledge of obstetrics desirable for such positions.

Statistical Study

A committee composed of the heads of the obstetrical hospitals in New York with Dr. E. H. Lewinski Corwin as chairman is now studying obstetrical statistics both in and out of the hospitals of the City. Under the auspices of the Hospital Information and Service Bureau this committee has already made a detailed comparative study of 4,000 stillbirths which have occurred in hospitals and their report will soon be ready for publication. They are also attempting to introduce a uniform nomenclature as it is impossible to consider live birth and stillbirth statistics

unless we have a standard definition of abortion and stillbirth. The Committee recommends the following definitions and hopes to have them adopted by all statistical registrars.

"The term stillbirth applies only to an infant born dead after the period of viability, that is the 28th week or 6½ calendar months or 7 lunar months of pregnancy, and which is approximately 1500 grams or 3 pounds in weight and about 35 centimeters or 14 inches in length."

"In recording a stillbirth a distinction must be drawn between a death before labor (antepartum), and during labor (intrapartum)."

"Fetuses born previous to viability shall be known as abortions; a distinction should be drawn between early and late abortions."

"An expulsion of the product of conception during the first 12 weeks of pregnancy should be considered as an early abortion."

"An expulsion of the product of conception from the end of the 12th week, or after the development of the placenta, and up to the 28th week should be considered as a late abortion."

The Committee hopes to provide also a statistical review on the deaths from sepsis and toxemia throughout the City. They hope in addition to have a certain group of hospitals operating with routine technic, in order to report the most favorable methods of treating certain obstetrical conditions.

COMMITTEE ON MEDICAL EDUCATION

ANNUAL GRADUATE FORTNIGHT
OCTOBER 1ST TO 14TH, 1928

PRELIMINARY ANNOUNCEMENT OF PROGRAM

The Problem of Aging and of Old Age will be the topic of the first Fortnight and will be discussed from many angles. Particular consideration will be given to the early recognition and prevention of disturbances which are often the underlying causes of aging. The program will include morning, afternoon and evening sessions each day.

SESSIONS IN TEACHING HOSPITALS

Special courses and clinical lectures and demonstrations have been arranged in over forty of the teaching hospitals of the city. (9 to 12 a.m. and 2 to 4 p.m.). For the purposes of the Fortnight the hospitals of the city have been divided into three groups. The hospitals of each group will present programs similar in character and scope so that it will be possible for the physician to spend a full day in the hospitals of one section of the city and thus avoid the necessity of making long trips between hospitals.

Among the subjects which will be presented in the hospital programs are:

Allergy, arthritis and orthopedics, blood chemistry, bronchoscopy, cardiology, dermatology and syphilology, diabetes, functional diagnosis, gastro-enterology, gynecology, hypertension and nephritis, neoplasms—cancer, neurology, ophthalmology, otolaryngology, pathology, physical therapy, pneumonia and infectious diseases, proctology, radiology, surgery, traumatic surgery and urology.

SESSIONS AT THE ACADEMY OF MEDICINE

During the Fortnight there will be two sessions daily at the Academy, one in the late afternoon (5 to 7 p.m.), the other in the evening (8:30 to 10:30 p.m.). Supper will be served at the Academy for those desiring it.

The list of speakers includes Sir Farquhar Buzzard, Pierre Janet and Vittorio Putti, from abroad, and the following Americans:

Harlow Brooks
Lawrason Brown
Samuel A. Brown
Thomas R. Brown
Leo Buerger
Alexis Carrel
Russell L. Cecil
Arthur F. Chace
Alfred E. Cohn
Charles F. Collins
Louis I. Dublin
William Engelbach
James Ewing
Nellis B. Foster
Howard Fox
Menas Gregory
Foster Kennedy
Alexander Lambert
Samuel W. Lambert
Emanuel Libman
Edward Allen Locke
Frederick T. Lord
Jerome M. Lynch
George M. MacKee
John E. MacKenty
Harrison S. Martland
John J. Moorhead
Herman O. Mosenthal
Bernard Sachs
Charles R. Stockard
Solomon Strouse
Frederick Tilney
George E. Vincent
Alfred S. Warthin
Benjamin P. Watson
Franklin W. White
Linsly R. Williams
William R. Williams
William H. Wilmer
Francis Carter Wood
John Wyckoff
Edwin G. Zabriskie

Among the subjects which will be discussed by the speakers are: Postponement of the individual processes of aging; pathological processes in aging; clinical aspects and management of old age; importance of anatomical pathways in diseases of middle life and old age; arteriosclerosis and aneurism; the relation of disorders of the ductless glands to senescence; traumatic surgery; bronchitis and asthma; physical therapy and climatology; hypertension; the psychoses; the myocardium; angina pectoris; arthritis; gastro-enterological problems; aging of the human brain; diet and body weight; apoplexy.

No fees will be charged for attendance at any of the clinics or meetings on the program.

RECENT ACCESSIONS TO THE LIBRARY

Adrian, E. D. The basis of sensation.
Lond., Christophers, 1928. 122 p.

Alsaker, R. L. Outwitting old age.
N. Y., Grant, 1927. 307 p.

Association for research in nervous and mental diseases. Schizophrenia (dementia praecox).
N. Y., Hoeber, 1927. 491 p.

Beaumont, G. E. & Dodds, E. C. Recent advances in medicine. 4. ed.
Phila., Blakiston, 1928. 426 p.

Bennett, H. E. School posture and seating.
Boston, Ginn, 1928. 323 p.

Blum, S. Practical dietetics for adults and children in health and disease. 3. ed.
Phila., Davis, 1928. 380 p.

Brocq, A. J. L. Clinique dermatologiques. 2. sér.
Paris, Masson, 1927. 659 p.

Brooks, R. O. Critical studies in the legal chemistry of foods.
N. Y., Chem. cat. co., 1927. 280 p.

Brousseau, K. Mongolism.
Balt., Williams, 1928. 210 p.

Bruehl, C. P. Birth-control and eugenics.
N. Y., Wagner, 1928. 249 p.

Brumpt, E. Précis de parasitologie. 4. éd.
Paris, Masson, 1927. 1452 p.

Burn, J. H. Methods of biological assay. . . .
London, Milford, 1928. 126 p.

Clinch, H. G. The smoke inspector's handbook; or economic smoke abatement.
London, Lewis & co., 1923. 136 p.

Cabot, R. C. Social service and the art of living. Rev. ed.
N. Y., Dodd, Mead & co., 1928. 214 p.

Calmette, A. L'infection bacillaire et la tuberculose . . . 3. éd.
Paris, Masson, 1928. 883 p.

Cazzani, U. La ipodermoterapia nella tecnica farmaceutica e nella pratica medica.
Milano, Ist. sieroterapico Milanese, 1928. 563 p.

Chopra, R. N. & Chandler, A. C. Anthelmintics and their uses in medical and veterinary practice.
Balt., Williams, 1928. 291 p.

Clinical pediatrics. Supervising editor, Royal Storrs Haynes.
N. Y., Appleton, 1928. v. 11-15.

Cotte, G. Les troubles fonctionnels de l'appareil génital de la femme.
Paris, Masson, 1928. 570 p.

Damon, S. R. Food infections and food intoxications.
Balt., Williams, 1928. 266 p.

Desvernine, C. M. Etude sur le traitement de la tuberculose pulmonaire chronique.
Paris, Arnette, 1928. 98 p.

Eisenberg, A. A. Principles of bacteriology. 4. ed.
St. Louis, Mosby, 1928. 228 p.

Ellis, W. B. Sanity for sale.
Advance, N. C., Advance pub. co., 1928. 179 p.

Emerson, C. P. Physical diagnosis.
Phila., Lippincott, 1928. 553 p.

Ewing, A. J. Why the Volstead law should be modified.
Kansas City, Mo., Burton, 1927. 68 p.

Fielding, M. Parenthood: design or accident?
London, Labour pub. co., 1928. 96 p.

Foote, J. A. State board questions and answers for nurses. 6. ed.
Phila., Lippincott, 1928. 491 p.

Foote, J. A. Materia medica and therapeutics for nurses. 4. ed.
Phila., Lippincott, 1928. 415 p.

Foster, N. B. The examination of patients. 2. ed.
Phila., Saunders, 1928. 392 p.

Franklin, B. Essays of . . . moral social and scientific.
N. Y., Putnam, 1927. 180 p.

Freud, S. The problem of lay-analyses.
N. Y., Brentano, 1927. 316 p.

Graham, E. A.; Cole, W. H.; Copher, G. H., & Moore, S. Diseases of the gall bladder and bile ducts.
Phila., Lea & Febiger, 1928. 477 p.

Gun, W. T. J. Studies in hereditary ability.
Lond., Allen, 1928. 288 p.

Hallock, G. T. & Turner, C. E. Health heroes. Louis Pasteur: Edward Jenner.
Bost., Heath, 1928. 2 vols.

Hamilton, A. E. This smoking world. . . .
Lond., Methuen & co., 1928. 178 p.

Harris, H. W. Human merchandise.
Lond., Benn, 1928. 272 p.

Humphris, F. H. Artificial sunlight and its therapeutic uses. (4. ed.)
Lond., H. Milford, 1928. 306 p.

Hunter, A. Creatine and creatinine.
Lond., Longmans, 1928. 281 p.

Kendrew, W. G. The climates of the continents, 2. ed.
Oxford, Clarendon pr., 1927. 400 p.

Kennedy, F. & Stevenson, L. Your nerves and their control.
N. Y., Appleton, 1928. 172 p.

Keynes, G. A bibliography of the writings of William Harvey, M. D., discoverer of the circulation of the blood.
Cambridge, Univ. pr., 1928. 67 p.

Kraupa, E. Der Glasbläserstar.
München, Bergmann, 1928. 119 p.

Ludovici, A. M. The night-hoers or, the case against birth-control and an alternative.
Lond., Jenkins, 1928. 288 p.

McCarrison, R. The simple goitres.
Lond., Baillière, 1928. 106 p.

Maier, H. W. La cocaine. Histoire, pathologie, clinique, thérapeutique, defense social.
Paris, Payot, 1928. 493 p.

Maxfield, K. E. The blind child and his reading.
N. Y. Am. found. for the blind, 1928. 215 p.

Mind (The) of the growing child. Edited by Viscountess Erleigh.
N. Y,. Oxford pr., 1928. 229 p.

Moltke, O. Contributions to the characterization and systematic classification of Bac. proteus vulgaris.
Copenhagen, Levin, 1927. 196 p.

Muhphy, J. Primitive man; his essential quest.
London, Milford, 1928. 341 p.

Myers, J. A. Fighters of fate.
Balt., Williams, 1927. 318 p.

Nessler, C. The story of hair.
N. Y., Boni, 1928. 277 p.

Osborn, H. F. Man rises to Parnassus.
Princeton, N. J., Princeton univ. pr., 1928. 250 p.

Pacchiai, P. Guida dell'ospedale Maggiore di Milano.
Milano, Cerelli, 1926. 372 p.

Peake, H. & Fleure, H. J. Peasants & potters.
New Haven, Yale univ. pr., 1927. 145 p.

Peake, H. & Fluere, H. J. Priests & kings.
New Haven, Yale univ. pr., 1927. 307 p.

Petty, O. H. Diabetes; its treatment by insulin and diet. 4. ed.
Phila., Davis., 1928. 155 p.

Polano, D. Geburtshilflich-gynäkologische Propädeutik. 5. Aufl.
Leipz., Kabitzsch, 1928. 224 p.

Pryde, J. Recent advances in biochemistry. 2. ed.
Lond., Churchill, 1928. 379 p.

Rand, B. Correspondence of John Locke and Edward Clarke.
Cambridge, Harvard univ. pr., 1927. 605 p.

Robertson, W. & Porter, C. Sanitary law and practice. 6. ed.
Lond., San. pub. co., 1928. 815 p.

Roland of Parma (surnamed Capelluti). La chirurgia di.
Roma, Ist. naz. med. farmacole. 1927. 28 ff.

Ruckmick, C. A. Mental life.
N. Y., Longmanns, 1928. 253 p.

Schlapp, M. G. & Smith, E. H. The new criminology.
N. Y., Boni, 1928. 325 p.

Shears, G. P. Obstetrics, normal and operative. 8. ed.
Phila., Lippincott, 1928. 745 p.

Soiland, A. Cancer.
N. Y., Appleton, 1928. 143 p.

Sorrin, S. & Miller, S. C. Practice of periodontia.
N. Y., Macmillan co., 1928. 160 p.

Taylor, A. S. Taylor's principles and practice of medical jurisprudence. 8. ed. Ed. by S. Smith. With a....rev. by W. G. H. Cook.
Lond., Churchill, 1928. 2 v.

Taylor, G. Environment and race.
Lond., Milford, 1927. 354 p.

Taylor, W. S. Morton Prince and abnormal psychology.
N. Y., Appleton, 1928. 136 p.

Teulie, G. Les rapports des langages néologiques et des idées délirantes...
Paris, Picard, 1927. 164 p.

Thompson, C. J. S. Mystery and lure of perfume.
Phila., Lippincott, 1927. 247 p.

Unconscious (The). A symposium. (By) C. M. Child, Kurt Koffa, John E. Anderson, et. al.
N. Y., Knopf, 1928. 260 p.

Veidovsky, F. Structure and development of the "living matter."
Prague, Royal Bohemian soc. of sci., 1926-7. 359 p.

Vernede, C. H. Geschiedenis der Ziekenverpleging.
Haarlem, Bohn, 1927. 400 p.

Vigano, L. Practical serology....Tr. by E. M. Heffer. Ed. by C. G. L. Wolf.
Cambridge, Heffer, 1928. 221 p.

Watson, J. B. & Watson, R. Psychological care of infant and child.
N. Y., Norton, 1928. 196 p.

Whitmore, E. R. Keeping young after forty.
N. Y., Appleton, 1928. 207 p.

Why men fail. Edited by M. Fishbein & W. A. White.
N. Y., Century co., 1928. 344 p.

Windle, C. A. The case against prohibition.
Chic., Iconoclast pub. co., 1927. 197 p.

Worrell, W. H. A study of races in the ancient near East.
Cambridge, Heffer, 1927. 139 p.

Wright, W. Muscle function.
N. Y., Hoeber, 1928. 188 p.

JEFFERSON MEDICAL COLLEGE AND HOSPITAL

The Committee on Archives of the Jefferson Medical College and Hospital of Philadelphia will appreciate gifts of, or information concerning, paintings, etchings, drawings, silhouettes, busts, photographs or cartoons of physicians who have served the hospital as internes or have been members of the staff of either college or hospital. Etchings, wood cuts, photographs or similar souvenirs of college or hospital buildings of the past will be gratefully received. All gifts or information concerning potential gifts or loans may be addressed to the Committee on Archives, care of The Chief Resident Physician, Jefferson Medical College Hospital, Philadelphia.

DEATHS OF FELLOWS OF THE ACADEMY

ARTHUR WALKER BINGHAM, M. D., 15 East 76th Street, New York City; graduated in medicine from the College of Physicians and Surgeons, New York City, in 1900; elected a Fellow of the Academy March 4, 1926; died, June 9, 1928. Dr. Bingham was a member of the Roosevelt Hospital Alumni Association, a member of the Society of Alumni of Sloane Hospital for Women and Attending Physician at the Willard Parker Hospital.

SIMON JOSEPH WALSH, M. D., 134 West 86th Street, New York City; graduated in medicine from the College of Physicians and Surgeons, New York City, in 1879; elected a Fellow of the Academy May 5, 1904; died June 11, 1928. Dr. Walsh was a member of the American Medical Association, a Fellow of the American College of Surgeons, Visiting Surgeon to the St. Vincents and Columbus Hospitals and Consulting Surgeon to St. Josephs, Far Rockaway and St. Josephs in Yonkers Hospitals.

ANSEL IRWIN MILLER, A. B., M. D., Brattleboro, Vermont; graduated in medicine from the College of Physicians and Surgeons, New York City, in 1884; elected a Fellow of the Academy December 7, 1905; died June 15, 1928.

JONATHAN WRIGHT, A.B. M. D., Windy Rock, Pleasantville, New York; graduated in medicine from the College of Physicians and Surgeons, New York City, in 1883; elected a Fellow of the Academy December 5, 1889; died May 26, 1928.